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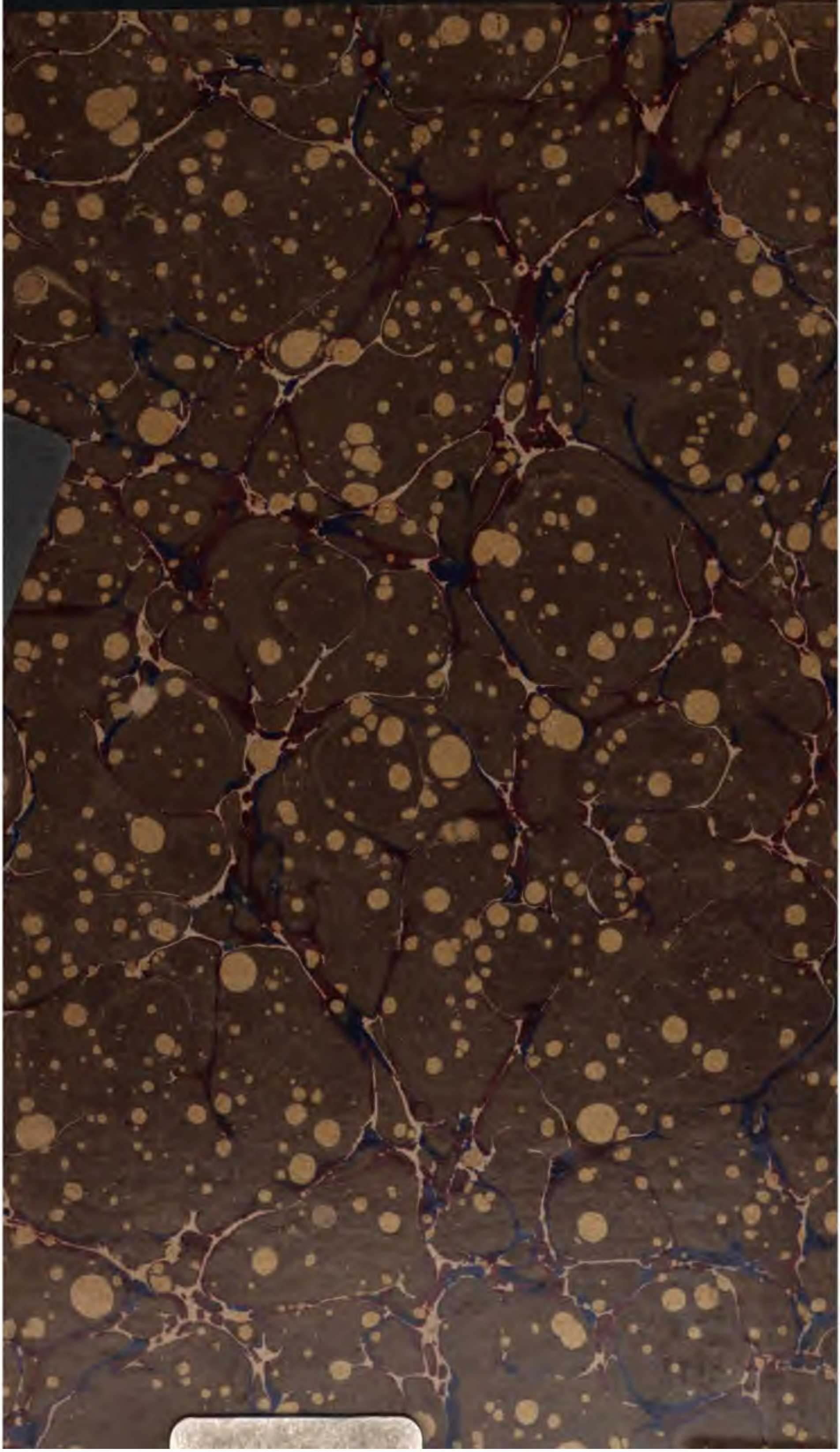

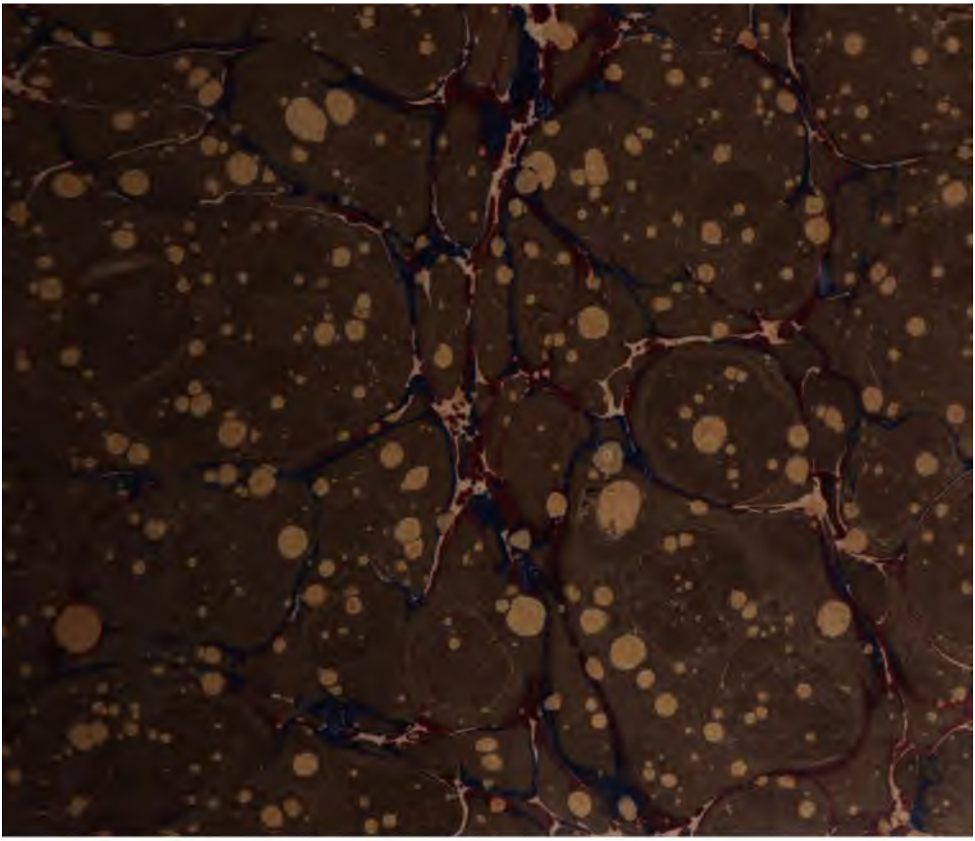

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THE PSYCHOLOGY OF THE SIMPLE ARITHMETICAL PROCESSES: A STUDY OF CERTAIN HABITS OF ATTENTION AND ASSOCIATION.

By CHARLES E. BROWNE,
Late Fellow in Psychology, Clark University.

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INTRODUCTION.

It is my purpose in the following paper to present the results of an experimental study of the simple processes of addition, subtraction, multiplication and division. These processes have been studied as they take place in the minds of that large class of people whose dealings with numbers in the ordinary affairs of life do not involve a high degree of practice. The study is therefore an inquiry into the psychology of the processes of adding, subtracting, multiplying and dividing as practically unchanged products of primary education.

In the experiments that follow eight fellow students in the Department of Psychology at Clark University (Drs. Fred Kuhlmann and Edmund B. Huey; Messrs. F. A. Lombard, L. M. Terman, J. R. Jewell, A. A. Cleveland, W. F. Book, T. Kuma) together with the writer, have served as subjects.[1] All are college graduates, but without special mathematical training. Kn. is strongly visual and auditory in mental type, H. is strongly motor but visualizes fairly, L. is slightly more auditory than visual but also has relatively strong visual and motor imagery; he is strongly impressed with the exactness of the numerical processes. T. and B. are predominantly motor with auditory imagery prominent, visual less so. J. is strongly motor; auditory imagery very prominent, visual exceedingly weak; has had experience in book-keeping. C. is predominantly visual; motor and auditory imagery also prominent; has had experience as a primary school teacher and some as a book-keeper. Bk. is strongly motor; auditory and visual imagery less pronounced; recalls "looking up answers in the back of the book." In later experience this lack of independence has developed the habit of proving results. K. is strongly motor and auditory, but visualizes fairly; earlier number training was received in the primary schools of Japan.

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[1] The writer wishes to express his obligation to these gentlemen, to the University and its Faculty in general, and to Professor E. C. Sanford in particular, under whose direct supervision the study was made.

Great care was taken to avoid both fatigue and effort, conscious or unconscious, to increase the efficiency by the practice involved in the experiments. The time was always taken chronographically to 0.1 sec., though only a subsidiary use of the results will be made in this paper.

I. ADDITION.

There were two series of experiments in adding: one in which the subjects proceeded by single digits; and the other in which combinations of digits were possible.

SINGLE-DIGIT ADDING.

A pack of about twenty cards, a single digit written upon each, with a cover card, was held by the subject in his left hand. At a signal he removed the cover card with his right hand and saw the first figure, removed the card carrying that, saw the second figure, added it; and so on to the end of the series, handling the cards as a card player would do in running through the pack to see that all were there.[1] The removal of the cards was practically automatic from the first and much more rapid than the adding. The figure to be added always appeared with the readiness to add and without conscious effort on the part of the subject.

The figures were added silently, the total sum being announced by the subject as a signal that the series was completed. The experimenter then recorded the introspections and time. The digits 1-9 inclusive, twenty of each, evenly distributed in ten packs, differently arranged at each sitting, were added five times over in five different sittings by each subject. Kn., K., L., J., T., and B. served as subjects in this series.

Adding digits in isolated pairs differs greatly from adding digits in series. The time in the latter case is by no means as even as Arnett's results would seem to indicate.[2] A wide range of variation is usually present. The average times per digit added were as follows: Kn, 1.09 sec.; K, 1.67; L, 1.28; J, 1.00; T, 1.03; B, 1.02. Average range of variation per digit added:[3] Kn, .37; K, .67; L, .36; J, .45; T, .59; B, .39.

Imagery. The terms in which the adding was done were in all the subjects largely motor, or motor-auditory (linguistic). Visual imagery, though present, seemed less fundamental to the process. Thus, Kn. whispers visibly, though not audibly, as a rule. K. has a strong movement of the lips accompanied by an almost constant strained undertone. The longer he is in reaching a result, the more intense this vocal accompaniment becomes. He constantly hears himself in his imagination pronouncing the sums. In J's adding lip movement is slight. He is conscious of holding his breath and of its expulsion at times as a prolonged "n-sound." L's adding is strongly motor accompanied by auditory imagery. Sometimes he uses the full tabular form, as "8 and 6 are 14," etc. Having to clear his throat, or cough slightly, arrests the adding.

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[1] The cards were highly glazed playing cards, left blank upon the face, and proved extremely easy of manipulation.

[2] Arnett: Counting and Adding. *Amer. Jour. Psy.*, XVI, 1905, 335.

[3] Each of the ten packs was added through five times. The difference between the longest and shortest time of adding through each pack was taken and the sum of these differences for the ten packs divided by 180 (number of digits).

In all cases difficulty in adding greatly increased the tendency to motor expression, which sometimes became explosive. Two of the subjects, when the results did not speedily appear, found themselves repeating the verbal formula over and over, as "54 and 9 are," until the result suddenly appeared. Any slowing of the process tends with four subjects to introduce the mental enunciation of the addition tables. In the other two, it causes a more strongly accented and more conscious saying of the results.

The partial sums were always given motor expression by all the subjects, though it did not often amount to audible speech. This was by no means a matter of mere habit or accident, but a fixed and definite part of the process, serving the useful and necessary purpose of subconsciously objectifying and holding fast the sum attained while attention was given to the next digit to be added. In the partial sum thus incipiently pronounced, the stress was on the digit rather than the ten: (to indicate accent by heavy faced type) 4**6**, 5**4**, 6**3**, etc.

The Adding Consciousness. In order to understand better what follows, it may be well at this point to forecast a general conclusion confirmed by the study as a whole. The adding process is initiated by the assumption of what may be termed an "addition set" or attitude of mind corresponding to the intention to add. The "addition set" is not, itself, in consciousness to any observable degree while the adding is in progress. Its office is to direct the association processes and hold them within the addition field.

Simple addition may be considered as a process of four stages: (1) A distinct consciousness of a number to which another is to be added; (2) the recognition of this other number; (3) the associative process leading to the sum of the two; (4) the distinct consciousness of that sum. In a continuous series (4) is obviously the same as (1) in the next step. The *recognition of the result*, initiating the innervation for its motorization;[1] and the *recognition of the digit to be*

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[1] The writer uses this and other similar terms as abbreviations for "more or less complete expression by incipient movements of speech."

added, as a rule not motorized, are the focal points of attention in the adding process. The associative stage is subconscious and seems to involve the following: (1) Motorization of the results (always present). In cases of difficulty or retardation of the process this motorization tends strongly to revert to the full verbal form of the addition tables. (2) Beside the motor accompaniment, and like it subconscious, is a continuous undertone of feeling upon which is based in large measure the adder's conviction that his work is correct or incorrect.

Except in temporary uncertainty, or distraction, the attention is directed forward, the associative process being left more or less to itself. It is clear that any imperfection in either phase of the attention, or in the subconscious stage of association, may result in error. Of the three phases, the most vulnerable is the stage of association.

Special Tendencies to Error. The numbers themselves in their incidental relations to each other, order of sequence, etc., became a frequent source of errors. These occur (1) where the numbers themselves rise into clear consciousness; (2) where they are less or entirely unconscious but produce the same effects. The following cases from the protocol will illustrate the tendency in question: $63 + 7 = 79$; the thought that 7 makes 70 was confusing. $26 + 7$; because 7 and $6 = 13$, the thought of the 1 in 13 made the subject say 31. $47 + 7$: 4 got into subject's mind and he wanted to say 11. $16 + 6 = 22 + 4 = 26$: confused, because "it seemed as if there were too many 6's." $79 + 2 = 87$: $5 + 5 + 4 = 19$, etc.

The following general conditions of false association appeared:

1. Numbers and results so arranged as to suggest the multiplication series (number repeated), particularly when the results, for some time follow the multiple series, and then fall just over or under it.

2. Numbers and results suggesting the counting series, as 1's in succession; or, when the digit of the successive results and the digits to be added fall in the ascending order of the count, as $21 + 2 = 23 + 4$, etc.

3. Dissimilarity in size between the digits added and the resulting digit, as $57 + 4 = 61$.

4. Similarity between the digit of the preceding result and the digit to be added, as $82 + 2$.

5. Or, in general, when any preceding digit remains in or near the focus of consciousness (as often happens when there is uncertainty regarding the accuracy of a result, or, in case the attention has been called in a particular manner to a particular digit), such a digit is likely to displace or change the digit of the result which is, or should be, in the focus of consciousness at that instant.

6. A tendency in the adding process to run ahead of the motorization leads to a frequent form of error. The attention moves on to the digit to be added before the motorization of the previous sum has taken place, with the result that the perceived digit is substituted for the digit of the sum.

General Tendencies to Error. Numerous cases of errors due to other causes than those just mentioned were in evidence. One subject is beset by the thought, for example, that he has made a mistake two or three figures back; or there may be a sort of running comment of questions as to the why of this or that, or of criticism or conjecture as to the sum total in the series being added, etc.; or the side current of thought may have reference to things entirely remote from the matter in hand.

Aside from these cases of distraction there were also occasional instants when the mind seemed a blank. So far as the condition can be judged from introspections, the mind does not wander, but associations fail and there is complete oblivion of any sort of imagery. The phenomenon appears to be closely related, if not identical, with what Mosso terms "dispersed attention."[1] The adding simply stops and there is entire inability to proceed. It is taken up again without apparent difficulty in the same place, though the adding usually goes harder and more slowly after one of these breaks. As a frequent secondary effect the subject feels that he has lost considerable time in the pause, and is likely to make an effort to increase his speed for the remaining part of the series, which in turn may result in distraction.

The Sense of Accuracy. The ultimate criterion of accuracy appears to be affective recognition. It is distinctly a feeling and not mere intellectual assent, which may often be observed dissociated from the feeling of accuracy. In such a case it seems to the subject as if he knew the result to be right, but the feeling of accuracy, or assurance, is lacking. Throughout the adding of each series, the feeling tone of certainty, or much oftener of doubt, is a constant accompaniment.

In a previous section we have distinguished, as a necessary focal point in the adding step, the recognition of the digit to be added, following the recognition and innervation of the previous sum and at the same instant that the subconscious motorization of the latter is taking place. It is in this subconscious motor process that the feeling of assurance is lodged. It is itself normally subconscious, but in case of saying a wrong result, the feeling of error often rises into the focus of attention, displacing entirely the normal recognition of the digit to be added while the error is consciously corrected. It was the general experience of the subjects that any difficulty in the adding or, even an error corrected, is almost sure to give a tone of uncertainty to the remaining part of the series. The feeling of the possibility of error often causes conscious reinstatement of parts of the series to see if the right result has been said.

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[1] Fatigue, p. 181. Dr. Kuhlmann and I myself noted the same phenomenon in a far more marked degree in adding figures continuously at maximum rate of speed in experimenting on practice and fatigue.

General conditions augmenting the sense of accuracy are uniformity of time, or rhythm; smoothness in the series; and easy combinations. Aside from repetition of the process with stronger, or with completely audible, verbal expression, no standard of absolute certainty appeared.

The Sense of Time. In so far as the sense of accuracy or assurance in adding depends on motorization, the element of time becomes an essential condition. In general, the subjects reported that, if the adding goes too fast or too slow, the sense of accuracy decreases. In the former case the motorization of results is slurred over; the time is not sufficient. In the latter case, while there is ample time for motorization to affect consciousness giving rise to the feeling of assurance, it is counterbalanced by the fact that the attention has between its normal focal points more time to wander. Accidental arrangements of digits and results become more suggestive of false association. The mean seems most important for accuracy. Motorizing of results in exact rhythmic intervals, assisted by other motor accentuations falling at the same time, was found helpful. With all the subjects any considerable deviation in uniformity of time arising from difficult combinations was always disturbing. The subject usually feels as though he must "catch up with" or "make up" the lost time. In case of error he is often in the dilemma of feeling that he *ought not* to go ahead, but that he *must not* stop. If he stops to correct his error he is distracted and harassed by a hyperæsthesic sense of "losing time;" if he goes on he feels uncertain of his accuracy.

Relative Ease and Difficulty of Combinations. It hardly need be said that any of the digits with 1 combines with the greatest ease. This is the simplest case of an easy combination. If many 1's are scattered throughout a series, the series as a whole tends to become less conscious, and the other combinations easier to deal with.

Even numbers appeared easier to combine than odd numbers, or than odd and even, for all the subjects. An intrinsic reason for this can be seen. All even-digit combinations and results are closely interrelated by common factors (always by 2 or a multiple of 2) and this common factor 2 also relates all such combinations and results with the direct count by 2's. In the odd-digit combinations this common factorial bond is not present. An exception is the doubling of odd digits, as 3+3, 7+7, etc. Here the process falls back upon the multiple series which psychologically is counting. The same may be said of 9+3, in which the common factor 3 closely relates it to the 3-count.

The simplest case of adding is the count by 1's, where the advance is made by a fixed unit at each step. The next simplest, and exactly analogous, case is the count by 2's, to which the combining of even digits is closely related. The greater ease in dealing with all possible combinations of the four even digits (2, 4, 6, and 8), is apparently due to a subconscious resolving into, or resting back upon, the 2-count, which itself is similarly related to the count by 1's. In adding (and the same was found true of the other simple arithmetical processes) the reinforcement of association for any given step, is roughly proportional to the number of other parallel or underlying processes leading to the same result. This gives a closely knit synthetic system easily reducible into simple common elements. There is a feeling of greater familiarity, because of the greater number of easily accessible links of association. Such an association complex is lacking with the odd digits, except as noted above.

While adding odd digits to each other is, in general, harder than combining even digits, it is still harder to add even and odd digits. A series of even digits yielding odd results (the series, of course, beginning with an odd number) generally proved very disturbing to all the subjects. To one subject 8+5 presented a standing challenge; he frequently found himself on the watch for it with a determination to "know that it was 13 if it should appear." At other times when he came upon an 8+5 combination he felt that, having had so much trouble with it, it was nearly useless to attempt to do much with it, and so counted on the five instead of adding it. With 63+4, after struggling a while with it, the subject counted on the four. Great difficulty was had also with 7+4 and 9+4 in remembering which was 11 and which 13, etc. The only exception found to the inherent difficulty of this class occurs in 6+3 and 9+6 related to the 3-count.

Relation of the Size of the Digit Added to the Difficulty of Combining. It is much more the size of the smaller of the two digits combined than the mere fact of large digits which determines relative ease or difficulty. The shorter the step in adding any two digits irrespective of the size of one of the digits the easier the process. It was the common method of the subject to add the smaller digit to the larger. Arnett reports the same.

Ebbinghaus's study of the memory of nonsense syllables is so applicable to some of these simple number relations that his conclusions are of interest in this connection.[1] He concludes, in substance, that in the process of impressing any series of ideas upon the mind by repetition, bonds of association are formed between all the individual members of the series. Every member of such a series acquires a tendency to bring the other members along with it when it re-enters consciousness. These bonds or tendencies are of different degrees of strength. For remote members of the series, they are weaker than for neighboring members. The associative bonds for given distances backward are weaker than for the same distance forward. The strength of all bonds increases with the number of repetitions. But the stronger bonds between neighboring members are much more quickly strengthened than are the weaker bonds between more distant members. Therefore the more the number of repetitions increases, so much stronger become these bonds absolutely and relatively to those of more separated members. On the basis of time saved in relearning six sixteen-syllable series, Ebbinghaus estimates the strength of the associative bond between contiguous and separated members of a series as follows—the percents are relative to the time required to learn the series at first:

	Time Saved.
Between contiguous members	33.3 %
Skipping one syllable	10.8 "
" two "	7.0 "
" three "	5.8 "
" seven "	3.3 "
With permutation	0.5 "

Our data shows a similar relation for the number series in the four simple processes here considered. The smaller of two digits added becomes the middle term or connecting link between the larger digit and the sum. In terms of Ebbinghaus's experiment—the strength of the associative bond between the larger of two digits and the result is inversely as the size of the smaller digit; or directly proportional to the difference between the digits; thus, in $9+2=11$, the associative bond between 9 and 11 is comparatively strong, but between 2 and 11, as $2+9=11$, it is very weak. Applying this scale, two distinct tendencies appear for which the introspections afford a considerable body of evidence: (1) The easiest combinations will be those in which the greatest disproportion between the digits exists as $7+2$, $9+2$, $8+3$. In general, combinations will be harder in the increasing order of the smaller of two digits giving the same result; easier in the increasing order of the larger digit. (2) Continuing up the range of possible combinations with constantly decreasing differences to the point where the difference between the two digits combined is least, as $4 + 3$, $5 + 4$, $8 + 7$, $9 + 8$, the difficulty of combining should be greatest; and curiously enough, we seem to find here the same law of the shortest step also operative—adding by subtracting 1 or 2 in the case of 9's and 8's from the smaller of two digits, and saying the result in the 'teens, especially common in 9-combinations. With other odd and even digit combinations in this class where only a difference of 1 exists, as $8 + 7$, $6 + 5$, and much less consciously with $5 + 4$, the addition is greatly reinforced and frequently comes directly from the doubling of the larger and subtracting 1. Thus $8 + 7 = 8 + 8 - 1 = 16 - 1$, etc. It is easy to add 6 and 5 because the sum is just 1 short of the familiar doublet, $6 + 6 = 12$, and also 1 greater than the more direct $5 + 5 = 10$. While $6 + 5 = 11$ is comparatively easy, $7 + 4 = 11$, as reported by the subjects, is most difficult. To formulate a somewhat general rule:

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[1] Ueber das Gedächtnis, Leipzig, 1885. Summarized by W. H. Burnham, *Amer. Jour. Psy.*, Vol. II, pp. 587-606.

1. The ease of combining is directly proportional to the difference between the two digits combined.
2. Where the difference between the two digits is very small, particularly where there is only a difference of 1 the combining is made comparatively easy by subtracting the difference between the larger digit and 10 from the smaller of the two digits which process becomes a cue for saying, in case it be 7—1, six-"teen" instead of six.
3. The most difficult digits to combine, where no common factor is present, are those falling between the two extremes, where the difference is neither very large relatively nor very small. These are: $7 + 5$, $7 + 4$, $8 + 5$, $8 + 3$, $9 + 5$, etc. To this list of most difficult must be added "class 2" in all cases where the adding is done directly.

The 'Teens. The decimal system represents a series of 17 members, in which the strength of the bonds of association steadily decreases, as the possibilities of distance between the members increases. From 1 to 10 the associative bonds are sufficient to admit of all combinations with comparative ease and certainty. In the "teens" associations weaken. Below 10, the longest step in combining is limited to 4 members, (except $5 + 5$ which belongs to a different order, the 5-count, and is not much harder than $1 + 1$. Transcending the 1-10 range, the largest number of members which may occur between any two is 8; $9 + 9$ reinforced by the multiple series is of the nature of a count; and $10 + 10$ is also a count. $10 + 9$ or $10 +$ any digit tends rapidly to become purely a cue for the immediate saying or writing of 19, etc. No conscious adding or combining is necessarily present. This holds for all 20's, 40's, 50's, etc., plus any digit. All results derived, in a way analogous to results in the range 1-10, as 12 + 4, 13 + 5, 45 + 4, etc., approach in degree of ease the simple 4 + 2, 5 + 3, 4 + 5, etc. This reduction leaves a range of results 11 to 17 inclusive, derived by combining single digits, psychologically differentiated from the lower range in respect to the character of the associative bonds. The subconscious recapitulation of the 1-10 range and of the "teens" is not only the frame-work, but very largely the substance, of all adding. The tens are transcended as a subconscious count. It is primarily with the digit relations that the adding consciousness has to do. Whether the adder is in 40's, 60's, or 90's, the consciousness of the series in this sense is very much submerged. A subject having 8 + 8 = 16 + 9 had to stop and think to himself 9 + 6 are 15; having 47 + 6 had to stop and say almost audibly 7 + 6 are 13. "All adding seems like a continuous referring to the numbers under 20," etc. Because of this constant subconscious reference, any habitual difficulty which one has in the "teens" is bound to be repeated in all adding as often as the troublesome combination recurs. Thus, if one has a strong tendency to confuse 7 + 4 = 11 and 7+6=13, the same difficulty is experienced with all 24's, 34's, 74's, etc., + 7, or 37's, etc., + 4, and similarly with all 7's + 6 and 6's + 7.

Summary.

1. *The adding psychosis* may be divided schematically into steps, each corresponding to a digit added. In each addition-step there are two focal points of attention and a subconscious association stage; the recognition of the digit to be added, the subconscious associative elements leading to the new sum, the recognition of this sum, which goes over into a pulse of innervation for its vocal expression (usually incipient).

The recognition of the digit to be added is not the recognition of 7 or 9, as such, but the recognition of the digit as tied to its subconscious associates. Chief of the associates just preceding and simultaneous with the focal flash is the motorizing of the previous result which at the instant of recognition is "still ringing," as the subjects often expressed it. The associates directly following comprise all that is gone through in adding the recognized digit.

2. *The subconscious fringe*, *i. e.*, the associative stage, may be wide or narrow. In proportion as it is wide, the attention may be directed to many other matters, arising either from central distraction or accidental conditions of the process. These were found to be: (a) Wandering of the attention;

irrelevant thoughts and images. (b) Slight fatigue effects where for an instant consciousness seems to be a blank and no imagery of any kind can be recalled.

3. *Accidental relations of numbers and results* may rise into consciousness as direct distractions, or produce their effect unconsciously as causes of error. (pp. 5 f.)

4. *The sense of accuracy*, or assurance, seems to be closely dependent on the subconscious motorization of the results. It is essentially a feeling and not mere intellectual assent.

5. *Any feeling of uncertainty* occurring in any part of the series usually imparts an uncertain tone to the remaining part of the series.

6. *Anticipation of results and combinations of digits* tend to arise in proportion as any particular digit or arrangement of digits is repeated.

7. *Ebbinghaus's laws of association* were found to hold in single digit adding. The reason for this appears in the fact, that adding, as Mach and others hold, is derived from the count, much evidence for which has been found in this study. The child's learning to count is analogous to Ebbinghaus's learning of nonsense syllables. Counting is purely a verbal formula, the one law being that the members always follow one another in the same order. The laws of association found by Ebbinghaus hold much more for the count than for nonsense syllables, in that, different from Ebbinghaus's standard of efficiency—being able to go through the series once or twice without error—the child goes through his series of nonsense syllables, the *one*, *two*, *three*, etc., until it is impossible to say it wrong, thus fastening upon his after experience in adding a bondage to these laws, of which the introspective data gave evidence at every turn.

8. *The decimal system* represents a series of 17 members entailing a maximum range of 8 members over which the associative bond must operate in combining the larger digits.

COMBINATION ADDING.

In this experiment the subject added 50 single columns of 20 digits each. He announced his results, which were recorded by the observer, as he proceeded. At the end of each column the observer and subject together went over the adding, noting combinations, peculiarities, errors, etc. The subjects, Kn., K., L., and J., were instructed to add as naturally as possible, making no attempt to combine beyond their pre-established habits. Kn. combined in 39% of the additions; K, 51 %; L, 54%; J, 73%. The time gained by each subject per digit over the time per digit in single-digit adding was in inverse ratio to the per cent. of combining.

During this experiment (and the evidence will grow as we proceed through the other processes) it became clear that the incipient motorizing of results as the adder proceeds is a relic of the earlier motorizing of the addition tables, standing in the adding consciousness for the fully expressed verbal form. Hence the assurance attaching to it. In the last analysis the adder is only sure that his result is right when it reduces to the tabular form as "6 and 7 are 13." A further reason why practice has unconsciously selected the more or less incipient saying of results as a permanent part of the process has already been given (p. 4). Motorizing the result objectifies and holds it subconsciously, thus freeing the focus of consciousness for the recognition of the next digit or combination to be added. In single digit adding the imagery of the recognized digit to be added was visual (directly seen). It was not motorized (except as the subject reverted to the verbalism of the tables), but combined as a visual element directly with the motorized result. In combination adding a similar division of labor between imagery is present.

Sense of the Tens. In single-digit adding the tens are transcended as a subconscious count. (p. 11). In combination adding this vague sense of progressing by 10's furnishes a stimulus, but also tends to regulate the combining habit. Thus Kn. proceeds by combinations as near 10 as possible. K. breaks the digit, or combination added, into two parts, such that one of the parts added to the digit of the preceding result will give exactly the next higher even ten. For one trained on the Japanese abacus, this procedure is as simple and direct as to separate 3 beads from 5. It is entirely visual and apparently immediate. The same is true of the recombining with 10. The Japanese method obviates the difficulties of adding in the "teens." There are no $(7+4)$'s, $(7+6)$'s or $(8+5)$'s, etc., to surmount. While J's conscious purpose is to combine as many digits as possible at each step, his combination must not transcend the next higher ten, a general caution with all the subjects.

Prolonged and Suppressed Motorization of the Ten. With Kn., K., and J., as the vague sense of the ten of the new result arises, its motorization begins and continues slowly until terminated by the short and forceful motorization of its digit. The duration of the prolonged motorization of the ten is determined by the time required for making the combination. But the sense of the ten alone impresses L. as incomplete; hence its prolonged motorization is inhibited. This inhibition gives rise to a feeling of strain or tension which becomes more intense the longer the new result is delayed. As the new result is motorized the sense of strain is relieved. The longer the inhibition the more time is required to regain the feeling of equilibrium; because, as L. expresses it, "the strain is greater." The holding back of the saying of the ten of the new result often became very audible. Strained hissing sounds escape like the initial *f*, *th*, *s* and *t*-sounds of 40, 30, 60, 20, etc. Apparently it is this pent up motor expression which gives rise to the vague feeling tone of strain which L. subconsciously attributes to the mental operation as a whole. In his adding each combination is followed for a few figures by single-digit adding until the feeling of mental poise is again reinstated, when another combination is attempted.

A graphic record of the time relations in saying the results was obtained by the use of a Morse key and revolving drum. The subject's left hand rested upon the key, and he easily learned to press it automatically (with neither attention nor effort) as he announced the successive results of his adding. Kn., K., and J.[1] pressed the key so far as could be determined simultaneously, with the beginning of their saying of the long drawn out ten. The key was released not simultaneously with the saying of the digit of the sum, but a little later. This slight pause seems like a brief waiting for the sense of assurance to arise. Then followed a much longer pause, during which the sense of the new ten was arising, terminated in turn by the pressing of the key at the beginning of its slow enunciation. With L. (and in the latter part of the experiment with K. also) motorization of both digit and ten took place as a sort of explosion, accent strongly on the digit, and the drum record shows but a single stroke. This distinction of two characteristic ways of motorizing results holds for each of the four processes and for writing the results in subtraction, multiplication and division L's case would also seem to indicate (as later evidence will tend to confirm) that inhibiting motorization necessitates a longer pause for the feeling of assurance to arise.

Kinds of Errors. The more common types of errors were found to be:

1. Skipping tens. This kind of error seems to arise from a vague sense of violation of uniform progress by 10's. If the subject passes over a ten, as $18 + (6 + 6) 12 = 30$, he is liable to feel that a step has been missed and in some part of the series to drop back into a lower ten. Or, if his combination leaves him still in the same ten, the impression may arise of being a ten behind and result in substitution of a higher ten at some point in the series.

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[1] A slight modification was used in J's case to accommodate the registration to his habit of using a pencil for keeping his place in the series.

2. When a digit is combined or added out of its natural order, the skipped digit is liable to be forgotten and not added; or, because of the ease with which the skipped digit is later combined with an even ten, it may be added almost unconsciously, but the feeling of its not having been added may persist so strongly that it is added a second time.

3. Momentary holding over in motorization of the ten and suppression of the digit of the result, while seeking to combine with it some other digit just ahead, is liable to result in the error of merely motorizing the digit instead of combining it with the suppressed digit.

4 A relatively small class of errors arises from the subconscious attempt to correct some slight error which has supposedly been made by adding or subtracting 1 from some digit (8 added as 7 or 9). The adder is liable to add when he should subtract, and *vice versa*. Errors of this kind are most common where 9's are added as "1 less" and 11's as "1 more" than 10.

Summary.

1 The main phases as summed up for single-digit adding will be found also in combination adding.

2. Two distinct types of motorizing of results were demonstrated: (a) As soon as the sense of the ten arises its motorization begins, and is prolonged while the digit part of the result is being derived either by combination or simple addition, when the long drawn out ten is terminated by the short and accentuated motorization of the digit. (b) In the second type, motorization is repressed till the idea of the complete result arises, when both ten and digit are motorized together. The repression of motorization produces in the subject a sense of strain followed by the feeling of relief as the result is motorized.

3. Four general types of errors were found.

4. After the motorization of each result a distinct pause occurs, the pause apparently affording opportunity for the sense of assurance, following upon the motorization, to arise.

II. MULTIPLICATION.

Subjects: T., H., B., and C. The numbers upon the cards used in single-digit adding were multiplied digit by digit consecutively, by each of the digits 2 to 9, inclusive, by each subject. The multiplier was announced an instant before the "ready" and "go" signals. Other conditions were the same as in single-digit adding. In the multiplying of T., H., and B. there were many traces of the tabular verbalism, and these were more prominent with the larger multipliers. With these any difficulty or halting of the association invariably threw the subject back upon the full motor expression, as, "six 8's are 48."

Any distraction of the attention did the same, as also did general fatigue. In C's multiplying hardly any trace of the verbalism was to be observed, the digit as it appeared on the card immediately suggesting the product. He found almost no difference in the ease or difficulty of the multiplying between the large and small multipliers, and his average time per digit agreed with his introspection. We should recall that C. represents a relatively high degree of practice.

Perfect certainty of individual multiplications and of the series as a whole was the rule—a radical difference from the feeling in adding. The motorizing of the results seemed, to all the subjects, essential to this feeling of accuracy. With H., B. and T. a marked tendency to run ahead of the incipient saying of the results was observed. Sometimes, apparently, the motorizing of the results was two digits behind the multiplication. The subject always felt this running ahead of the motorizing to be a danger; a uniform time per digit seemed necessary. If any result is retarded one of two dangers is liable: (1) The subject subconsciously feels that he must hurry the multiplication to avoid a break in the uniform time of motorizing the results which becomes a distraction that further hinders the rise of the desired result. In such a case the whole process was liable to be held up at that point and the subject proceed slowly for all the rest of the series. (2) If the process does not halt on the digit whose result is retarded, it frequently happens that hesitancy allows the motorizing process to overtake the multiplying process. In this case the retarded result may not be motorrized at all, the motorization passing directly to the next result. The subject in such a case feels that he must go back and say the result whose motorization has been left out, although perfectly certain, intellectually, of its correctness, the same phenomenon is to be observed in simple dividing. C., whose time was the shortest and most uniform for all the processes, did not show this tendency to run ahead of the motor series. He felt that he must say each result before passing to the next multiplication and so dispose of it.

"*The Multiplication-set.*" In adding, some indication of an "addition-set" or attitude appeared. A slight *Anregung* was also present. The "set" is much strengthened by the first steps in the series in each of the four simple processes. In simple multiplication the "set" is intimately connected with anticipation and its principal element is the multiplier. T. and B., on hearing the multiplier announced, frequently found themselves thinking over the possibilities in the upper part of the table. All of the subjects were in a state of expectancy while waiting for the multiplier to be announced and gave it special attention. C., B., T., would at times, find themselves repeating it.

During the multiplying the multiplier was an almost (if not entirely) unconscious element in the process. Even when the verbalism appeared, the multiplier seemed of little consequence. Whether the formula runs six 1's are 6; six 2's are 12, etc., or once 6 is 6, 2 times 6 is 12, etc., the multiplier in either case is psychologically the constant, and serves only as the sign of a particular set of associations.

The digit part of the result did not stand out clear and distinct in consciousness from the ten as in addition, but each product seemed to the subject a whole. The accent appeared to be solely determined by the laws of rhythm and euphony, depending on the habit of saying the tables.

WRITTEN MULTIPLICATION.

Subjects: T., H., Bk. and C. Eight examples written upon white cards, each containing all the digits differently arranged, were multiplied by each digit, 2 to 9 inclusive, by each subject. The card, held in place by the left hand, rested across a tin plate so arranged that the writing of the digits on the card pressed down the plate sufficiently to bring it into contact with a row of brass screws directly under it, thus completing an electrical circuit, and recording the time of writing each digit upon a rotating drum. The example was covered by a piece of paper removed simultaneously with the signal "go."

In general, not knowing the multiplier and not being able to anticipate made the work slower at the start than with the cards. The subject tended to start before the particular "set" was established. The general procedure of the subjects may be summed up somewhat as follows:

First a brief hesitancy while the "set" or attitude for the particular table called for is establishing itself. Along with this arises an affective tone, its character depending on the size of the multiplier. The multiplications are as easy and sure as in the card multiplying. The subject is, however, thrown back more upon the full motor expression of the tables.

While writing the digit there is often a tendency to look back upon the immediately preceding step. The writing begins with the first faint inkling of what the digit is to be, and progresses slowly until the subject is perfectly sure of his result. Often the subject will begin to write the wrong digit and unconsciously change it to the right one without lifting the pencil. This slowness is principally confined to making the first part of the digit. With the clearing up of the idea or the assurance of its correctness, the unfinished part of the digit is executed rapidly. But Bk. inhibits the writing until sure of correctness, when the digit is rapidly and forcefully executed. This method seems to favor or necessitate a slight resting pause involving a slight sense of relief as the previously inhibited writing of the digit takes place.

With the smaller multipliers where the possibilities of carrying are reduced, the tendency is to perform the multiplying and carrying in advance of the writing. This also occurs with the larger multipliers, where the numbers to be multiplied are small. Here both multiplying, adding and carrying are so much simplified that the digits of the complete product come quickly and easily. The motorization of it is apparently reduced, and it is handed over, at once, to automatic writing, while the attention is left free to run ahead. In the first case where multipliers are large the subject seems to be "thinking with his pencil;" in the simplified case, the writing of the digit follows the actual multiplying and carrying as an entirely detached and automatic part of the process.

Two types of difficulty arose in connection with carrying: inability to carry (T.); inability to add (H., Bk.).

The cause of the difficulty in carrying is two-fold: (a) In adding, as we saw, a division of labor takes place between motor and visual imagery. A visualized or a perceived digit was combined directly with a motor foregoing result. Here there seems to be a clash of motor images. The motor ten of the foregoing product is to be added to the following motor product. The economy of the division of labor between imagery (at least as far as simple numerical relations are concerned) seems to be that a motor and a visual image of two different digits to be added, say, may exist simultaneously, one of course being much less conscious than the other at any given instant. In the case of two motor images this is impossible, for while such images may follow each other, if the attention turn back for the first, it can only be revived as the other image is displaced. This appears to be one of the chief causes of difficulty in the carrying of multiplication and will probably explain the trouble experienced with carrying generally. (b) From a feature of adding already pointed out, the tens are very subconscious, the attention being mainly upon the digit. But in multiplying the ten is quite as important as the digit. One must attend to it in order to have it for carrying. Hence arises an inconsistency in the process itself. The subject whose attention in normal adding lets the tens take care of themselves and focuses upon the digit relations, must, in multiplying, attempt to keep both ten and digit in attention while he adds, and this apparently is possible only when a third kind of motor imagery is made use of, the writing of the digit. For C., the writing of the digit served this purpose. But for the ordinary multiplier this is not the rule in so far as the other three subjects are typical of that class. If the subject adds in the normal way, letting his ten go (T.), when he needs his ten to add he will find that it has slipped away. If he remembers that he must retain the ten (H., Bk.) so as to be able to carry it on and add it to the next product, he will find it very difficult to add, as his attention is thus drawn away from the digit, which one must of necessity attend to in adding, although he can readily carry. We may expect then, in general, to find these two types among ordinary multipliers, and also a large class of characteristic errors arising from each source.

C. did not, like the other three subjects, experience particular difficulty in either carrying or adding. He recalls earlier difficulties of this nature, but practice has apparently eradicated them. In other respects, except being on the whole much more immediate, his method does not essentially differ from the foregoing account.

The degree of reversion to the more primitive tabular expression, found with all the subjects, was in general proportional to the size of the multiplier. As the size of the multiplier increases, the possibilities of carrying are also proportionally increased. In itself it is not hard to use a large multiplier, but it proves so because of the increased range in carrying. With 2 as multiplier, if any digit is carried, it is always 1; hence carrying means "count 1." With 3 also, the carried digit will often be 1 and never be more than 2; 4, as multiplier, means a range 1 to 3 in carrying; 5 is unique, in that, while its range is 1-4, the product to be added to is always 5 or 0. In case it is 0 it is a mere cue for the saying of the digit carried; no real adding is involved; hence the greater ease of 5, as compared with 4, as multiplier. The difference between the average time per digit in written multiplication and that of simple multiplying with the cards, in general, increased proportionally with the size of the multiplier.

With the 2's, 3's and 5's full motor expression does not arise in C's multiplying. As C. expresses it, the multiplication "set" is not interrupted as with larger multipliers, owing to the few possibilities of carrying. But from the interruption of the multiplication "set" which occurs with the multipliers above 5, motorizing the full tabular form is frequently necessary to reinstate the "set." All C's products are strongly motorized. The adding is a very conscious part of the process. He does not motorize the carried digit except as it is represented in the strongly motor ten—**5**7, say. The 7 is written automatically and not further attended to. Thus the attention, freed from the digit, focuses upon the ten, as the digit to be carried. Auditory imagery is especially prominent.

Summary.

1. *The imagery* is predominantly motor and auditory. Visual imagery also is present, but apparently plays a subsidiary rôle. The great predominance of motor-auditory imagery is due apparently to the difficulties of carrying (*i. e.*, motorization of results in adding, and the auditory-motor form of the multiplication tables).

2. *Carrying*. The most vulnerable part of written multiplying is the carrying. In adding we normally hold the results in terms of motor imagery while the attention passes to the following digit (visual) to be added. In carrying, the digit to be added comes first, the product to which it is to be added second; thus the conditions of normal adding are reversed.

Two types of difficulty arise from carrying: (a) As was pointed out in a previous section, the attention is most concerned with the digit relations, and very little concerned with the tens in normal adding. The multiplier who adds in the usual way is in constant danger of losing the ten (digit to be added). (b) The multiplier who realizes this danger will try to retain the ten by making it more conscious, thus drafting away the attention from the digit of the result, and will experience great difficulty in adding. In type (a) adding is relatively easy, carrying difficult. In type (b) carrying is relatively easy, adding difficult.

3. *Writing the digit* is practically automatic. With practice it tends to become a detatched system of imagery, which is very submerged in consciousness. The motorization (linguistic) of the digit tends to fall away and disappear, and the writing tends to take its place. The ten more and more tends to receive all the motorization which in normal adding goes largely to the saying of the digit. Both digit and ten thus tend, with relatively high practice, to be objectified and held until each has properly functioned. The effect might perhaps be induced with a lower degree of practice, if consciously attempted.

4. *Pedagogical*. From the foregoing data, three pedagogical inferences seem warranted.

(a) The difficulties superimposed upon the relatively easy process of multiplying by carrying would be entirely obviated by the method of writing the entire products at each step (multiplier placed at the left, the writing progressing in the natural way toward the right and adding at the end of the process). Psychologically this method is much simpler than the common method involving carrying. A similar statement should also be made for the same general method applied in adding where carrying is involved, for much the same reason.

(b) The importance of thoroughly mastering the verbal formula of the tables lies in the fact that the formula may be easily revived after long lapses of practice. The tables furnish an instrument, always at hand, for the determining of any product within their range with a certainty proportional to the proficiency with which the tables have been mastered.

The multiplication tables are also a helpful standard of reference in addition, where digits are repeated, or where the adder can single out at a glance all the 8's, say, in a column. The adding may thus become much easier, and the adder absolutely sure of correctness when $8+8+8$ yield 24, because three 8's are 24, etc. The multiplication tables are also indispensable to division.

But in proportion to the number of steps in the verbalism is the number of members which must be weeded out as the process approximates immediate association. Hence the verbal formula should be of as few words and as suggestive of immediate associations as possible. Although in general such a verbalism presents more or less of a barrier to immediate association, there is some compensation in that it may operate in a very subconscious manner, thus freeing the attention for other parts of the process. In case of momentary withdrawing of the attention, fatigue, etc., this part of the process may be relied upon practically to take care of itself.

The use of tables, however, as a basis for deriving results is not only slow, but fatiguing, because, unlike visual imagery which is relatively instantaneous, motor imagery involves a distinct process.

Another element of fatigue is the inhibition of saying the tabular form aloud. The subjects frequently and quite generally reported a strained feeling in the throat accompanying the thinking in terms of the tables; the tongue pressed against the teeth, or roof of the mouth, etc. A further slight cause of fatigue is the inhibition of motorizing non-essential members in the verbalism as the tendency to immediate association begins to short circuit the process.

(c) The Multiplication Tables. Multiplication is abbreviated addition. The child should not study addition and multiplication as two distinct subjects, but understand the latter as a special case of the former, that he may make as large a use as practicable of the reinforcement which the multiple series ought, psychologically, to lend to the adding.

In so far, however, as the addition point of view enters as a subjective factor into the tables, either consciously, or subconsciously, suggested by the form six 8's etc., it is likely to introduce a new factor affecting the ease and difficulty of multiplying. It will be easier to think three 9's than nine 3's.

Thinking in terms of the tables will be harder as the multiplier becomes large with reference to the digit multiplied.

Since it is the office of the multiplier to suggest strongly, or switch the mental processes into, the particular set of associations (table) called for, it should stand first in the verbal formula. The multiplier should always be smaller than the digit multiplied. To think in terms of the smaller digit times the larger offers no necessarily greater difficulty than the immediate recognition: $9 \times 3 = 3 \times 9$. Such a method would reduce the multiplication tables about half. The larger tables would almost disappear, *i. e.*, the twelve-table would go entirely; $\frac{11}{12}$ of the eleven-table; $\frac{5}{6}$ of ten-table; $\frac{3}{4}$ of the nine-table, $\frac{2}{3}$ of the eight-table; $\frac{7}{12}$ of the seven-table; $\frac{1}{2}$ of the six-table, etc.

The cases in which both factors are the same should probably be learned as a separate table, because they form a unique series, the square and square-root system. From their tabular form they constitute a table where it is possible to reduce the verbal formula to a minimum, thus greatly favoring immediate association. In the multiplication tables 9 may mean any number from 18 to 72. In the square system 9 always means 81. We only need to say 9–81; 8–64; 7–49, etc.

The form suggested by traces of the tables which now and then rose to consciousness with our subjects and also favored by the addition point of view was, for example, six 8's–48, in which "*are*" has disappeared and *six* is tending also to disappear.

Addition and multiplication are essentially synthetic. The associative bonds operate forward. In subtraction and division the bonds of association operate backward, hence the two distinctly different classes into which the four simple processes divide.

III. SUBTRACTION.

Subjects: Kn., K., L. and J. Simple subtraction with the same packs of cards as in single-digit adding preceded written subtraction. No pack had digits adding 100, 99 being the highest sum, 79 the lowest. The subject always started at 100, subtracting each digit as it appeared from the previous remainder. Other conditions as in single-digit adding.

The subtraction "set" is harder to initiate than either the addition or multiplication "set." The interchange of motor and visual imagery is like that of adding. By Kn., L., and J., the perceived digit (on the card) is first attended to in the light of the preceding digit of the motorized remainder, which is being motorized at the instant of perceiving the digit. If the perceived digit is smaller than the digit of the previous remainder, the subtraction falling in the range 1–10, is relatively immediate. If the digit to be subtracted is larger, it leads to what may be called "subtracting by adding." The digit seen becomes a cue to begin motorizing the next lower ten strongly. The digit to be subtracted, say 7, is perceived while the previous remainder 53, is being motorized. Following the recognition of the digit comes the recognition, $7 > 3$, which is a cue for motorizing the next lower ten, 40. Now arises a rough formula "forty-something" and 7 are $53 = 6$, hence 46. This method, very general at first, should, however, be regarded as due to a relatively low degree of practice. Three principles, which later data will further confirm, were suggested:

1. Subtraction is harder than addition. Introspective evidence for this was general and the average time per digit subtracted was longer: Kn, 2.2; K, 2.6; L, 1.9; J, 1.5 (p. 3).

Ebbinghaus's law of serial association described in case of adding (p. 9) must, from the nature of the case, operate also in subtraction. From the fact that the count is repeated forward (counting backward as a habit is hardly comparable), the associations backward are weaker than for the same distances forward, decreasing in strength as the distance between members increases.

2. For this reason, a balance will be struck between adding and subtracting. For the adding, it will naturally tip in favor of subtraction (as we found) when the distance forward is great enough to make the associative bond weaker than subtracting over a much shorter step ($8 + 7 = 16 - 1$; $9 + 8 = 8 - 1 = 7$"teen," etc.). But when associative bonds in the forward direction are stronger than in the backward direction, subtracting (or verification) by adding becomes common; so much so, that that subtraction may be regarded as a derived process from adding.

3. But the fact of subtraction as a process derived from addition has an important secondary result. The process of "subtracting by adding," while easier as far as the fundamental laws of association are concerned, is too roundabout in practice, and immediate association tends to come in more quickly. This was a marked feature of the card subtracting. A small amount of practice with all the subjects showed great gain in deriving remainders immediately and dispensing with addition. The same phenomenon also appeared in dividing, which bears a like relation to multiplication. Subtraction and division are far more immediate than addition and multiplication, though probably far less practiced. But the subject, as a rule, does not trust his remainders as they appear and only learns to do so after considerable practice. Subtraction and division, while the most immediate of the four simple processes, are also characterized by lack of confidence and consequent "proving;" and this is especially true of subtraction. As the adding gave

way to immediate association, it was still retained by Kn., L. and J. to prove the results.

K., as in adding, always breaks a larger subtrahend digit in two, in such a way that one part will give the ten; the other is then taken from 10. All his subtractions thus fall in the range 1-10. What was said in favor of his method in adding applies still more in subtracting.

WRITTEN SUBTRACTION.

Subjects: Kn., J., L. and K. The method was similar to that used in written multiplication. Fifty examples having each digit in both subtrahend and minuend, but differently arranged each time, were performed by each subject. The more important part of the data concerns the three different methods employed. K's method is Japanese. If the subtrahend digit is larger, it is taken from 10 and the remainder added to the minuend digit. Otherwise the method is the same as that of Kn. L. and J. use the older method of borrowing by adding 1 to the subtrahend; Kn., the common method of subtracting 1 from the minuend.

No evidence of the entire suspending of motor imagery for one of the digits (usually the lower, though often both are strongly motorized, as 6 from 13) appeared; but in simple taking away, there is a tendency for a glance at the two digits, involving mere appearance without recognition of either digit in itself, to constitute a cue for immediately writing a certain figure without motorization. Several other similar cues were also found: (1) Where the two digits, subtrahend and minuend, are equal. (2) When a difference of 1 exists, the subtrahend digit being larger, this relation may become an immediate cue for writing 9. (3) The subtrahend digit being larger by 2, may become a cue for immediately writing 8. But the difference of 2 yielding 8 immediately seemed a limit. Such cues work best when there is no borrowing. In case of borrowing, it also works fairly well in the method of J. and L. But reducing the minuend digit (Kn., K.) hinders the use of such cues in proportion to the degree of forgetting whether or not borrowing should occur.

The most difficult subtracting is where the borrowing is interspersed with cases of simple taking away. The habit of borrowing rapidly establishes itself and the subject is liable to continue it over into the simple subtractions and *vice versa*. Involved in adding to (K.) or subtracting from, the upper digit (Kn.) is the necessity of remembering whether the previous minuend digit has been raised to a "teen," or looking back to see. For K. and Kn. this was most difficult. The difficulty is twofold:

1. The memory as to borrowing is very feeble when it exists at all. Sometimes it seems to be retained as a memory of moving the pencil towards the next minuend digit, from which theoretically the borrowing is to be done, and also a general feeling of having moved slightly in that direction (K.), or as a memory of the visual 1 of the digit raised to the "teen" (J.); which vaguely retained sense means: "take away 1 from the minuend digit before adding to, or subtracting from it." From difficulty of recall and the vagueness of such imagery arises a strong tendency to look back each time to the foregoing step, just before the adding or subtracting occurs. The glance back, however, interposed as it is between two closely related parts of the process, is also vague; hence, it too, is unreliable. The lower digit is already being motorized, which is really the beginning of the adding, or subtracting, and the normal direction of the attention impels one to continue: now interposed between these naturally almost inseparable processes comes a hasty backward glance. In case this glance brings with it the impression of having to borrow, the adding, or subtracting step already begun is broken off and the minuend digit reduced by 1, and then the adding, or subtracting, is completed. The only looking back which can be relied upon occurs as the digit (remainder) is being written, which takes place so automatically that the attention is free to look back.

2. The greater inconsistency, however, occurs in decreasing the minuend digit. It is not difficult to reduce this digit by 1, but a confusion is apt to arise from the fact that ordinarily this digit operates in the process of adding or subtracting as a visual element. The attention moves up to it from the lower digit and as it is recognized in connection with the lower motorized digit, the addition or subtraction occurs, with the minuend digit directly within the field of vision. Hence arises the possibility of having to visualize a 4, say, with a perceived 5 directly in the field of vision.

If the 4 is motorized, the difficulty in holding it in motor imagery with the original 5 directly within the field of vision, may lead to errors allied to that class in addition where a perceived digit displaces a motor digit in the result. The motor 4 must also displace the natural and more economic division of labor between visual and motor-auditory imagery in the following subtraction. For K., the process is further complicated by having to subtract and add to the same figure at as nearly the same time as possible. Kn. by the ordinary method must perform a double subtraction, which proved quite as difficult as the "subtract-then-add" process of K.

When both upper and lower digit are alike, the more or less conscious backward glance gives no cue as to whether borrowing shall occur, which becomes a source of error, the subject assuming wrongly either alternative. J. and L. did not experience difficulties of the above nature. The diagrammatic comparison of the methods given below will make plain the reasons.

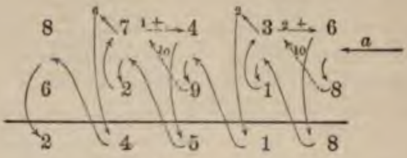

Fig. 1.

General movement of the attention. The following diagrams represent the general movement of the attention for each subject as determined introspectively. The arrows indicate the focal points in the general direction which the attention takes in moving through the subtraction steps. These are the points of conscious recognition, but, as already pointed out, there is no such phenomenon present as distinct flashes of the attention or disintegrated recognitions of the various important points in the process. Each recognition is an integral part of the one preceding; or the various subconscious effects, following the foregoing recognition, constitute the necessary associates of the following recognition. No true recognition can take place unless the accustomed preceding recognition has occurred. Like a few instances in multiplication and division, if a foregoing recognition fails to occur in its natural order, the subject finds himself, as he looks at the figures in the written form of the example, staring at symbols devoid of all meaning. The arrow (a) in K's method *Fig. 1*, indicates the first focal point, recognition of the relative size of the two digits, 6 and 8. Following this recognition the attention, indicated by the second arrow, moves to the subtrahend digit, 8, its recognition initiating strong motorization. The third and fourth arrows in broken lines indicate the vague movement of the pencil towards the 3 from which 1 is to be taken, and represent the taking of the 8 from 10, the recognition being a cue for the strongly motorized 2, which, as the attention moves to the recognition of the 6 (visual), is added to it. The fifth arrow represents the fifth focal point, the rising into focal consciousness of the resulting 8, initiating innervation both for its motorization and writing.

The next step, 3—1, may represent in principle the chief inconsistency of the method. The first arrow, indicating the recognition of the relative size of the digits, has obviated the necessity of taking 1 from 10 and adding 9 to the upper digit.

But as the attention moves to the recognition of 3, from the physiological memory of the general movement in the direction of 3 while taking 8 from 10 in the previous step, or more often looking back to see, as the 3 is recognized, the recognition involves reducing it by 1, and thinking it as 2. This requires a change in the focus of the atttention from 3 to 2. The difficulty arising, whether the 2 be visualized or motorized (p. 25), increases in proportion to the difficulty of the subtraction. The last subtraction 8—6 represents the simple case reduced to two focal points, recognition of the relation 8-6, immediately associated with the writing of 2.

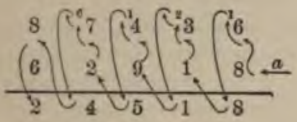

Fig. 2.

For Kn. *Fig. 2* (subtrahend motor, minuend visual), the first and second focal points, are invariable, except when the tendency to write a certain digit from the mere appearance of the two digits involved, becomes operative. The raising of 6 to 16 does not require an added pulse of attention, as does the reducing of the minuend digit. In the particular "set" of the preceding recognition 8 > 6, 6 is not recognized as 6, but as 16, a mentally visualized 1 taking its place beside the 6 as it is recognized. The difficulties of reducing the minuend digit, however, are increased in cases where the digit has to be raised to the "teen" and also reduced by 1.

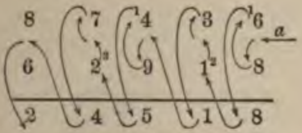

Fig. 3.

For J, *Fig. 3*, the first focal point is the recognition of the relative size of 6 and 8; the second embraces the recognition and motorization of 8; in the third focal point, 6 is recognized as at motor 16. J's subtractions are in motor terms. The fourth focal point completing the step, with 8 motorized and automatically written, the attention goes at once to the next digit, 1, which is recognized as 2 (subconscious count), the first focal point of the new step.

Slightly different is L's diagram (visual type) *Fig. 4*. His

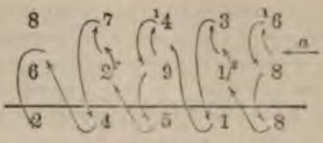

Fig. 4.

second focal point motorizes 16; the third visualizes 8, yielding the motor result 8, automatically written; the attention then goes at once to 1, raised to 2, by subconscious count. The 2 is strongly motor; but passing to 3, the visual imagery predominates. Again, in the third step, 14—9, 14 is the second focal point strongly motorized; the third focal point is 9 visualized; the fourth, the motor result 5, from which, while automatically written, the attention moves directly to the clearly recognized 3 (subconsciously raised from 2), and so on.

Certain errors and difficulties inherent in the method of borrowing commonly taught at present—the liability of forgetting whether borrowing should occur, the difficulties in reducing the minuend digit by 1, the characteristic errors and general inconvenience of looking back, together with the affective tone of doubt, which it creates at every step—are greatly reduced, if not entirely obviated, by the older method used by J. and L. When the common method is used with visual imagery, *Fig. 2*, the visualized 1 (placed before minuend digits requiring borrowing) must be entrusted to physiological memory until the next minuend digit is reached. From that digit it is separated by four focal points: the resulting digit of the remainder, the recognition of the next digit of the subtrahend, the recognition of the relative size of this digit and the corresponding digit of the minuend, and the the recognition of the minuend digit to be reduced. In case the minuend were motorized and the subtrahend digit kept in visual imagery, the attention moving downward from the upper digit to the lower, four focal points would still intervene. If the subtracting were entirely in motor terms, as "8 from 16 are," four focal points would intervene.

Let us now turn to the older method in which we find the three possible ways of proceeding represented in the diagrams of J., predominantly motor, *Fig. 3*, and L. predominantly visual, *Fig. 4*. In the first case where the upper digit is visualized as the "teens," which visualization is the basis of the physiological memory, only one focal point intervenes between such visualization and the actual increasing of the digit, as against four focal points intervening in the common method. In the case of the motorization of the minuend digit and the visualizing of the subtrahend digit, two focal points will occur. In the other possible case, the motorizing of both subtrahend and minuend digits, only one focal point will occur. A combination of the Japanese with the older method, eliminating subtracting from the "teens" in the older method and reducing the minuend digit in the Japanese method, would seem, from the psychological point of view, a distinct advantage.

As with the schematic description of the adding consciousness and that of multiplying, so here many modifications may arise, though the main focal points of the attention will always be found present. Attention may go off in a great variety of ways between the normal focal points. It may double on its course, go back and reinstate previous focal points; it may be influenced occasionally or habitually by subconscious doubt as to whether borrowing should occur, and so go over the foregoing step again and return with the missing associates. Any case of simple subtracting may prove to be an immediate perception of relation of size, leading at once to the writing of a particular digit always associated with this particular perception. The common habit of verifying the result as it rises into clear consciousness may cause the interposing of a complete addition step; and distractions from central causes are always liable.

Writing the Digits of the Remainder. The following general principles will be found to hold, with slight modification, for multiplication and short division as well as for subtraction.

1. The writing of the digit is automatic.

2. Doubt of the result or the habit of verifying may inhibit the writing of the digit until the verifying has taken place, in which case two things may happen: (a) The digit may be quickly and forcefully written as a kind of explosion of inhibited action, giving the subject a sense of relief and inducing a slight rest, which such inhibition seems to necessitate, before going to the next step. (b) The inhibited ideo-motor action may reach expression more slowly and occur during the preliminary recognitions involved in the next step, being terminated just before the actual subtraction is made.

3. If the tendency to begin writing the digit as the first vague idea of it rises into consciousness is not inhibited, which is more common, the subject is usually repeating the process or verifying it by a reverse process, while the writing is slowly progressing.

4. If assurance comes with the first focal recognition of the digit as in simpler cases, the time of writing the digit will be the time required to perform the next step.

5. Thus, the automatic writing of the digit follows the movement of the attention. If the coming in of the focal idea with its tone of assurance is delayed, so much slower propor-

tionally becomes the writing of the digit. If it clears up in consciousness quickly, the remaining unfinished part of the digit is executed with a rapid stroke. If the train of imagery connected with the clearing up of the idea in consciousness is intercepted by irrelevant imagery, the movement of the pencil stops.

Summary.

1. Simple subtraction is a derived process, being governed in its mode of operation by the laws of association which operate in adding and which in turn are derived primarily from the conditions of counting.
2. Subtraction is harder than adding because the associative bonds operate more weakly in the reverse order. For this reason children should learn to count backward as well as forward.
3. Hence arises the phenomenon of "subtracting by adding," which in practice becomes too cumbersome, and tends to disappear, giving place to immediate association. The adding formula, however, tends to persist in subtraction as a means of verification.
4. The older method of increasing the subtrahend appears to be superior to the present (more logical) method of decreasing the minuend, which is largely responsible for the difficulties of borrowing. (p. 24.)

IV. DIVISION.

Before the experiments in written short division, a series in simple dividing took place. Eight packs of eighteen cards each were used. Each pack contained all of the nine multiples of the digits, except 1, yielding a quotient 1 to 9, each multiple appearing twice. The subject divided through each pack, each time differently arranged, ten times. The divisor was announced just before the "ready" and "go" signals. Other conditions were the same as already described in the foregoing experiments. The subjects were J., Bk., H. and C.

Division is a derived process depending primarily upon multiplication in much the same way and for the same reasons as subtraction depends upon addition.

The original formulæ: as "6 into 34, 5 times, etc.," "18 divided by 3 gives 6," are only vaguely motorized as the work is performed, and apparently assist but feebly in the deriving of results, if at all. In C's dividing (relatively immediate) the process still seems like a "reverse" process; and this was more pronounced in the other three subjects. Many instances of reaching the quotient digit by way of the multiplication formula occurred, as 6 "somethings" are 54 = a shadowy 9, immediately verified by transposition into the verbalism, "six 9's are 54." This double formula represents apparently the primitive method. It is always the last but sure resort when the quotient digit fails to appear promptly.

As with subtraction, this cumbersome double formula tends rapidly to be short circuited into immediate association. Even in the short practice of 1440 simple divisions, these traces, quite evident at the beginning, grew appreciably less. But unlike the addition formula in subtracting, the multiplication formula does not appear to be retained for purposes of verification. It would seem that addition, most characterized by uncertainty of the four simple processes, passes on its own uncertainty to subtraction. Although the subject almost always finds his result right after proving, he does not therefore reason, consciously or subconsciously, that the result as it appears is probably right and so rid himself of the verifying habit. The habit, on the contrary, tends to persist. The subject, in the most pronounced cases, seems to revel in the sense of the certainty, so largely lacking in its parent process, addition, which use of the adding verbalism imparts to the subtracting step. The certainty of simple multiplication, seems also to impart something of itself to simple division; and any sort of verification tends to fall away. The character of the immediate association in division is quite different, therefore, from that in subtraction.

Kinds of Errors. No errors involving digits not factors of the number divided were discovered. Any factor commonly used as a divisor of a given dividend is liable to appear as the quotient digit, or even the divisor itself may so appear. Three kinds of such errors were found:

1. Errors making the divisor or some other factor of the dividend the quotient digit, as 24÷8 giving a quotient 8 or 4. Numbers such as 24, 16, 12, containing more than two factors commonly used as divisors, were especially liable to this kind of error. Such errors are insidious because the subject generally passes over them with no sense of inaccuracy. Most of them were discovered by the subject when a number immediately following gave the same result, as $24 \div 6 = 3$, followed by $18 \div 6$ also yielding 3; whereupon the subject remembers the previously motorized 3 with the vague recognition that the number which gave it was not 18. Even C. made errors of this kind.

2. Another sort of error arose in dividing a digit by itself, as $5 \div 5 = 5$.

3. When only a difference of 1 exists between the divisor and quotient digits, even if the divisor is normally present in the subconscious fringe, the subject frequently has difficulty

in selecting the required quotient. In $72 \div 8 = 9$, 8 and 9 are contiguous members in the counting series, and as one comes into consciousness it tends strongly, as earlier shown, to bring the other along with it, and this creates a sense of doubt as to which is really right. Cases of this kind are about the only instances of any tendency on the part of the subject to doubt the quotient digit as it appears.

The phenomenon of running ahead of the motorizing of the results, as in simple multiplication, appears in simple division also. But from the experience of the subjects a definite limit is evident about two recognitions in advance of the motorizing. If a third recognition occurs, the subconsciously following train of motor results is apt to be lost sight of. In such a case all becomes hazy, and the process stops. Having once gone two recognitions ahead of the motorizing series, any slowing of the recognition process is likely to result in a peculiar kind of confusion. The subject feels the subconscious motor series approaching and that he must keep ahead of it. Such a momentum has arisen from the necessity of its keeping up with the recognition series that when the recognition series slows up, this acceleration in the motor series induces forced recognitions and any of the foregoing kinds of error may result, or, as more often happens, complete confusion may arise. Conversely the checking of the motor series imposes a check upon the recognition series.

Written Short Division.

Subjects: T., Bk., H. and C. The method and examples used were the same as described in written multiplication except that the numbers were divided instead of being multiplied.

As in the preceding cases of multiplying and simple dividing, it is essential to attend to the divisor before beginning the example. A feeling tone arises from the initial attention to the divisor corresponding to its size. Getting well started (*Anregung*) requires one or two repetitions of the division process.

In dividing, the writing of the quotient digit occurs near the beginning of the division step. In multiplication and in subtraction the recording of the corresponding digits occurs near the end of the step. After the quotient figure appears (digit to be written), the subtraction is still to take place and the new dividend to be formed. This fact gives to division a very different character from that of the other two written processes. It greatly encourages automatic writing of the digit. The writing tends to go along as a parallel but separate series, requiring no attention. As the idea of the quotient digit arises into the focus of consciousness it is motorized, but handed over directly to the writing mechanism, while the attention, thus freed, passes to the subtraction of the product from the dividend number. The figure is written slowly, its time being determined by the time required to complete the step.

Although the double multiplication formula did not tend to persist in simple dividing, it became far more apparent in written division, especially in the more difficult cases where the dividend number is so far away from the multiple that it suggests it feebly, if at all. Let us take as a dividend 59, divisor, 8, multiple to be derived, 56, quotient-figure, 7. A vague multiplication formula appears as the subject attends to the 59 visually; as "something × 8 is something in the vicinity of 59." After more or less hesitancy 56 appears as this last wanting term. The subject may now have further difficulty in dividing 56, in any of the ways pointed out in the last section. Deriving the multiple 56 and the quotient digit 7 are both accomplished in motor terms.

If we work through a step or two of a section of an example the various phases may be more clearly illustrated. Let us take $6295 \div 8 = 786+$.

1. While the divisor 8 goes along, as a rule in the simple cases, as a very submerged part of the process, it is reinstated as often as the subject brings back the multiplication formula to determine the multiple and quotient digit. The subject has, let us say, already derived the first multiple 56, as just described, a process often characterized by the subjects as "feeling or groping about for" the number required. The 7, obtained thus by a motor procedure, is now to be written automatically. The subject begins to write the digit as soon as the first faint idea of what it is to be arises, often starting to make a wrong digit, but subconsciously changing it to the right one without removing the pencil. The 7 is motorized as the idea of it becomes focal, but it drops entirely from consciousness normally as soon as derived, and the automatic writing of it takes place as a disconnected part of the process.

2. On the instant of yielding the 7 (handed over directly to the automatic writing with the shortest and most incipient pulse of attention comporting with the degree of assurance) the strongly motor 56 is at the same instant being subtracted from the 62, directly seen and not motorized. In the subtraction, however, the minuend 6 is disregarded and 5, following the law also of all tens in adding and subtracting, is very submerged. A visual 1 has appeared at the left of the 2, the visually imaged 1 and the perceptual 2 giving 12. Thus the subtraction takes place yielding a strongly motor 6. This 6 is located below the 62. The writing of 7 is still slowly

progressing, its speed depending entirely on the time of the movement of the attention through the step.

3. The motor 6 felt as below 62, is now felt to move upward and take its place beside the 9 as a visual 6, and the motor image having been translated into visual terms, now disappears.[1] This visualized 6 and the 9 (directly seen) is the new dividend 69. The motor imagery, freed from the care of the 6, now initiates the motor process involved in getting back to 64, and from this 64 to 8, also motor, but at once handed over to the automatic writing. The same cycle is now to be repeated for the next step, and so on to the end of the example.

The motor parts of the dividing step were extremely pronounced for H., often to verifying of subtracted results by counting. Bk. represents an extreme type of the verifying habit. His quotients, often immediate, must be multiplied over again "to see if they are right," and he usually finds that they are; but he keeps on verifying as if the reverse were true. Very often remainders appear immediately, but are not to be trusted until verified by adding. He often gets his remainders directly by adding, in which case they are verified by direct subtraction. When he gets his new dividend, if it is large, it is liable to look "too large," which means that the whole step will be repeated "to see." The effect of this verifying habit upon the average time was marked.

As in multiplication, Bk. writes the figure with a quick, forceful stroke, the writing following the assurance. This induces a further disadvantageous consequence in the breaking off of the process to write the figure, for the main part of the division step is yet to be performed. It is not only bad in itself for the reason that it works against immediate association; but it is unduly fatiguing because of the excess of motor imagery which the verifying involves; it lengthens the time and prevents automatic writing. When the thread of the process interrupted by the writing of the digit, is again taken up, the product to be subtracted has often disappeared, and this involves the necessity of reinstatement. H. began by writing the quotient digit in a similar manner, but before the end of the experiment changed his method unconsciously to that already described.

Immediate Association. The type of immediate association in subtracting appears to be a little different from that where the mere appearance of two digits becomes a cue for the writing of a certain figure. Beside, not being written the digit is not an end in itself, as in actual subtraction, but a means. The point of view is that of deriving a digit for the ten of the new dividend. That is, it is immediate subtraction in the particular division "set." It partakes apparently of the kind of immediate association where attention to the multiple tends to produce in consciousness the quotient digit, the divisor being given. Let us take as an illustrative case, 6 from 11, represented by the accompanying diagram, *Fig. 5*. Although similar, the case is more complicated than attending to the multiple for deriving a desired quotient. There is, however, one compensating condition. In the case of the multiple, the given factor (the divisor) is extremely subconscious; in this case the digit is very close to the focus of consciousness, being motorized as the 11 is being attended to visually. As the 11 (the right hand digit being seen directly, the left digit visualized) is attended to, it tends to bring into consciousness some one of the digits with which it is associated as a sum. Moreover it tends to bring one of the digits because a digit is wanted for the purpose of the new dividend. The 6 is already present in strong motor imagery; hence that digit will tend to appear which is the associate of 6 in producing 11; and 5 will appear all the more quickly because 5 and 6 are contiguous members in the counting series, though in descending order. Many cases were reported by all the subjects in which the digit for the new dividend seemed "to drop out" of its own accord from the given number and take its place at the left a little above the dividend digit of the new dividend. In such cases there is attention to the given visual number alone, with no special thought of subtraction, exactly as the multiple is attended to in simple division. A similar diagram (*cf.* James: Principles of Psychology, Vol. I, p. 586) might represent the type of immediate association in deriving the quotient digit. Errors in yielding other factors than the quotient digit are more liable in proportion as the presence of the divisor in the subconscious fringe is weak.

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[1] These subjects have never followed or been taught the method in short division of putting the remainder before the dividend figure for the new dividend.

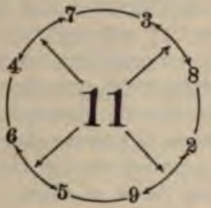

Fig. 5.

Relation of Dividend Number to Difficulty in Dividing. In multiplying, the larger multiplers seemed to increase the difficulty proportionally. This was not on account of the multiplying in itself, but because of the increased possibilities of carrying and adding. In division a similar relation seems to hold, but not because of the increased possibilities of the size of the digit to be subtracted, although this fact is not without influence. The chief increase of difficulty, arises from the corresponding increase of possible distance in the number scale between the dividend and the next lower exact multiple. The reported cases and general introspective evidence of all the subjects seem to warrant the following general statement:

1. The most difficult cases of deriving a multiple occur when the dividend-number falls just under the next multiple above; as 61, divisor 7. 63 is strongly suggested, but 63 is not wanted; 56 is the desired multiple. It was the opinion of the subjects that the attempt to get back directly to the 56, was the cause of what they felt to be the worst feature of division, "the feeling, or groping, for" part of the process. The strong and natural associative bond in the direction of 63, works directly against the attempt of the subject to get back by a chance hit to 56. Persisting in the point of view established both by education and practice, the subject can only flounder about vaguely for the desired multiple. If, as sometimes happens, he takes up as a last resort, with the natural line of association, going to 63, thus setting aside for the moment the dominating point of view, the finding of the multiple becomes comparatively easy, though somewhat roundabout. At least it is little harder than the method (multiplication) in which many of the quotients are actually derived; as seven 9's are 63, but one less is wanted, hence seven 8's, etc. At least there is a pedagogic advantage in this procedure, in that it works in the line of the natural associations.

2. The easiest cases occur when the dividend-number falls just over the desired multiple. Being thus so much farther removed from the multiple above, and nearer the one below, the dividend number tends to suggest the latter.

3. A region of associative dead-lock appears to occur with the larger divisors somewhere in the middle region between the two multiples. These cases are not so difficult as the first class because associative tendencies, up and down, neutralize each other. This difficulty, of course, disappears proportionally as the divisors become smaller, because the possibilities are always 1 less than the divisor used.

In these cases of increase in difficulty with increase in the size of the divisor (and similarly in multiplying, with increase in size of the multiplier), another factor is operative. It is allied to discrimination and choice in reaction experiments. Such experiments show that as the possibilities increase the time of reaction is lengthened. Most in point for our special problem is the study of Vintschgau[1] upon the times required in multiplication. His subjects reacted by giving the products, one of the two factors being given them in advance. As the smaller of the two factors was always announced first the subject hearing "nine" had but one possibility 9×9. If 2 was first announced, the range of possibilities varied from 2 to 9. The tables were run through in this way, and it was found that, in general, the length of time increased as the number of possibilities increased, 9×9 giving the shortest time.

Summary.

1. Division is a derived process based upon multiplication. Unlike subtraction, which still continues to be largely influenced by the point of view of addition, division tends to free itself from the point of view of multiplication and to develop a type of immediate association. It does not revert for verification to the multiplication formula, in a degree approaching the tendency in subtraction to revert to addition. While subtraction is a verifying and proving process par excellence, division is a process of immediate association.
2. The writing of the quotient figure occurs at the beginning of the division step, and this not only favors automatic writing of the digit, but practically necessitates it.
3. In written short division the step comprises three stages (pp. 33 f.).
4. The difficulty of the process as a whole increases proportionally with the size of the divisor, because of the increased range of possibilities as to the dividend numbers falling above the multiple (p. 35).

In the preceding sections I have considered chiefly the more fundamental aspects upon which there was practical unanimity in the experience of the subjects, or marked disagreement. The various experiments are of such a character as to be easily repeated. Any one wishing to put the foregoing inferences to a practical test, should have little difficulty in ascertaining to what extent these phases are present in his own experience and that of others.

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[1] *Pflüger's Archiv*, Vol. XXXVII, pp. 127-202.

THE TIME OF SOME MENTAL PROCESSES IN THE RETARDATION AND EXCITEMENT OF INSANITY.

By Shepherd Ivory Franz.

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Introduction. The present work was undertaken in order to gather material for the solution of the problem: to what part or parts of the nervous system may we refer the increased and the decreased psychomotor activity usually found in depressed-maniacal insanity.

The earlier psychiatrists considered as separate disease entities abnormal emotional depressions and exaltations, under the names of melancholia and mania, and the condition in which there is a more or less regular alternation of the exaltation and the depression, called circular insanity. The last state was deemed very peculiar, but until the time of Kraepelin it seemed not to have been closely associated with either of the two other conditions. To Kraepelin is due the credit for having shown that circular insanity, mania and melancholia (in its simple form) are interrelated and belong to one group which he termed *Manisch-depressive Irresein.*[1] Hoch has well summed up the main points of the present conceptions (according to the Kraepelinian school) of the combination of mania, melancholia and the older circular insanity;[2] a tendency to recurrence, the absence of mental deterioration, psychomotor excitability or retardation, emotional exhilaration or depression, and difficulty or (apparent) ease in thinking. These alternative symptoms may be variously combined in individual cases, the most usual, however, being the combinations of (a) exhilaration+excitability+flight of ideas and (b) depression+retardation+difficulty in thinking. In the excited phase of this condition the emotional tone is exalted. There is a feeling of *bien aise*, of expansiveness, of great ability, and of self-satisfaction. There is more or less motor excitement and a seeming rapidity of associations, with flight of ideas. The associations are usually superficial and sound associations. The attention is very unstable. In the depressed phase there is a lowering of emotional tone, and a feeling of *malaise*, of hopelessness, perhaps of fear, and there may be suicidal tendencies and attempts. There is often a profound motor disturbance in which the reactions —using that word in its widest sense—are slowed, and an appearance of decrease in associative ability; in other words a retardation. Any of these symptoms, but of course not opposites, may be present to a mild or to a most marked degree.

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[1] This name has been variously translated, the most common being "manic-depressive insanity." See Hoch's article in Wood's *Handbook of the Medical Sciences*, Vol. V, "Manic Depressive Insanity," Paton's recent book: *Psychiatry*, and de Fursac, *Manual of Psychiatry*, Trans. by Rosanoff. This designation is a transliteration of the German, regardless of English meaning and spelling; the word *manic* is not given in the Century Dictionary and the word *depressive* means "able or tending to depress." It is not the insanity which is depressive but the depression is the "insanity."

[2] *Op cit.*

The differences in motor activity of this class of the insane have been studied to some extent[1] but so far as I am aware there is no published account of work such as is here attempted. At the present writing there is available only an incomplete skeleton of the work necessary for a full understanding of these motor conditions. The present article is, however, complete in itself and is to be considered as part of the general plan.

Clinical Histories of the Subjects. Six subjects were chiefly used in the research, two normal, two depressed (retarded), and two exhilarated (excited), as follows:

Normal subjects. F., the writer, upon whom the whole series could not be made, was used as one normal subject. The other normal subject was B., a business man, age 49 at the time the experiments were being made, had been a patient at the hospital for about seven years. During the course of the experiments B. was considered 'recovered,' and had been practically so for about a year. He was admitted[2] to the hospital Oct. 22, 1897. He had previously indulged quite freely in alcoholic liquors, but for over a year had very little alcohol. For ten months before admission the patient had appeared not so well, had become absent-minded, depressed, and later more inactive. On entrance he spoke slowly in a monotonous tone, expression immobile, rather blank, yet said he worried about everything. He was not clear as to where he was, and in the afternoon did not remember the physician whom he had seen for a short time on entrance (morning). Later he was oriented as to time, place and persons, did not occupy himself, did not speak spontaneously, but answered calculation questions, even difficult ones, very promptly, but questions regarding his condition were answered by "I don't know." There was no memory difficulty. In four months he became a little more spontaneous, and his sentences were a little longer when he talked. In the gymnastic class he was reported to make his movements promptly and well. For four years he remained practically stationary, answering slowly, doing little, but when made to do things, as in the gymnasium class, doing them promptly and well. "It is the fact that with a sufficient external stimulus the patient can act promptly while it seems that if he has to supply the stimulus himself there is so little activity that it makes his general condition appear rather the outcome of an apathy than of a retardation." In June, 1903, it was noted that "there has been a slow, gradual gain which from day to day or week to week, or even month to month has been imperceptible, but which on looking back has amounted to a very great transformation. The man shows now in regard to conversation and in regard to occupying himself with everything that is possible in the hospital a very decided spontaneity. He shows, moreover, good judgment in his conversation. He is one of the best billiard, tennis and whist players and is occupied all the time." He did not ask to be allowed to go home, did not think he could attend to his business, but when questioned in regard to his condition the old reply "I don't know" was sometimes given. During the following summer the patient was occupied most of the time playing golf, tennis, etc. There was a gradual improvement, although when anything out of the ordinary arose he was inclined to hesitate a little. During the succeeding year there was a gradual recovery and at the time of the laboratory experiments he was practically well, although disinclined to take up business. The patient was discharged May 29, 1905.

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[1] Especially reaction times: W. Bevan Lewis, *Textbook of Mental Diseases*, 2d edit., London, 1899, pp. 164, 364 and 365. Richet: *Dictionnaire de Physiologie*, Article Cerveau, Vol. III, p. 29. v. Tschisch: *Neurol. Centralblatt*, 1885, IV, p. 217. G. Lefmann: *Psychol. Arbeiten*, 1904, IV, 603-668. On fatigue: Hoch, *Journ. of Nervous and Mental Diseases*, 1904, XXVIII, 620-628.

[2] The clinical accounts have been copied from the hospital records. The notes were made chiefly by Drs. A. Hoch, Steele, Roberts and Hamilton.

Retarded subjects. Ed., a hotel clerk, age 21, was admitted to the hospital May 8, 1904, greatly depressed and retarded. For some years previous to his attack of melancholia he had been troubled greatly with dyspepsia, and he had always been inclined to worry about his business and other affairs. In March, 1904, the patient could not attend to his work, gave up his position, and was greatly worried about himself. He would say, as an example of his depressive ideas, "I am no good." When taken to drive he feared lest the horse was not strong enough to pull him, etc. At this time the patient was taken to his father's office and performed what little things were asked of him, *e. g.*, writing down orders and answering the telephone, but there was no spontaneity. Gradually he became slower in his movements, but in the beginning of May was restless and once made an attempt at suicide by drowning. After this he was even less active, refused food, and was untidy if not closely watched. He expressed the ideas that he was turning to stone, that his throat had been cut, that his feet had been taken off, that he had no tongue, and that his neck was broken. After his entrance to the hospital he remained in bed, lying perfectly quiet, seldom moving any part of his body. Upon repeated requests he did not show his tongue, although it was seen to move slightly in his mouth, which was partly open. There was no reaction to pin pricks. He was tube-fed. When requested he could be made to walk slowly to a chair and to sit down, but all movements made at command were done very slowly and often incompletely. It was impossible to get the patient to answer any questions. On most occasions he had to be moved by the nurses from place to place. On one or two occasions he was heard to say "What have I done?" "Where am I?" About three weeks later the patient obeyed simple commands such as 'Get out of bed,' 'Sit in the chair,' but all his movements were made slowly, and with a considerable interval between the command and the beginning of the movement. Common answers to questions, sometimes given quite promptly, were "No," "I don't know." He knew his name and age, and related part of his previous history slowly but well, but he did not know where he was, how he came to the hospital, or how long he had been here. He did not remember the physician although he had been tube-fed by him for twenty-three days, three times a day. A few days later he could not be made to give the date, place, etc., and answered to all questions "I don't know." At this time he was heard to remark, "I have no eyes, no arms, no legs." Three weeks later he knew the names of the nurses and of many of the patients on the ward, and not infrequently made remarks about the latter. His time was entirely unoccupied, and was spent walking slowly about the hall or in the yard, or sitting or lying about the hall or in his room. On June 2, he ate well three meals and from that time did not have to be tube-fed. Simple questions were now answered readily, but questions the answers to which required the least thought either received no response or "I don't know." July 30, there was considerable improvement over his previous condition. Occasionally, especially towards evening, he walked about better, and talked more freely to the nurses and physicians. At times, however, all one could get from him was "I am suffering more than any one else in the world," or "I am all twisted," etc. On general subjects he often answered well but slowly, but so soon as his condition was touched upon, he would not answer. He was at this time perfectly clear regarding his surroundings, oriented as to time and place, and knew the people about him. On August 24th, when the series of experiments were begun, the patient was in the condition just described. He answered slowly and in whispers and it took considerable persuasion and often commanding to get him to start upon the work. A month later, September 26, it was noted that there had been a steady improvement in the patient's condition. He took part in games, and made a fairly natural general impression. He played pool and billiards well. For about three weeks he had not talked so much of being "abused" and of being "the sickest man in the world," and these ideas had been gradually disappearing. The period of his convalescence seemed to be very short; he seemed to get well by leaps and bounds. The experiments were continued from August 24 until his discharge "recovered" Nov. 9, 1904.

Ev., the other retarded subject, was at the time of the experiments 65 years old. He was a business man, admitted to the hospital Oct. 15, 1903. In temperament he was inclined to be optimistic. He was social and entertaining, but rather easily irritated. Previous to his present illness he had had six attacks of depression. All of these had been rather typical, showing depression, a feeling of inadequacy, and some retardation. These attacks came respectively at the following ages: 22, 30, 37, 44, 51, and 58. The later ones lasted about six months, and the course was very similar in all. Each started with a depression, and a gradual quieting and inactivity. Then he took little interest in things about him, was depressed about everything, thinking that his business was 'going to the bad' and that his family would be ruined. At these times there was considerable self-reproach, etc. He always spoke in a low tone and answered slowly. The attacks usually wore off quite gradually, with an improvement in his physical condition and a relief from his insomnia, and he returned to his 'natural optimistic self.' During the past five years the patient had been under a mental strain because of business difficulties. The present illness began in December, 1902. He began to be depressed and to sleep poorly, but kept at his business until February, 1903, when the firm failed. He was 'completely exhausted' by the failure and remained in bed for a week during which time his condition was as follows: quiet, felt unable to do things, had considerable self-reproach, took little interest in things about him, complained of feeling exhausted and weak. Then he became restless and agitated and would not eat. He was sent to an insane hospital in a neighboring city, his restlessness disappeared and gave place to a retardation. He remained quiet for the most of the time, sat in one place, and had to be urged to do things. He was mentally clear, and well oriented. On entrance to McLean Hospital, October, 1903, the patient answered questions very slowly—five to eight seconds—looked quite depressed, obeyed commands slowly, walked quite hesitatingly, protruded the tongue barely beyond the lips. He sat about unoccupied, without any spontaneous talk, almost with no movement. He showed, in fact, a very typical psychomotor retardation. He is senile physically, but not mentally. The condition of the patient remained stable. He was always quiet, saying and doing nothing except an occasional mumbled answer in a tone so low as to be scarcely audible. His time was occupied in standing quietly or sitting in one place, occasionally looking around him. He occasionally answered questions briefly, slowly, and in a very low tone. When told to go to the dining room he went very slowly, but was able to find his own seat. He ate in a normal manner, using knife, fork and spoon properly, and at the proper times. At night he would not undress himself, and in the morning would not get up until taken out of bed by the nurse. He ate and slept well. During the next few months the patient gradually improved in that he took more notice of things about him, moved a little more quickly, and more often. He looked after his wants better, and was a little more independent, *e. g.*, he would go to the dining room when the bell was rung. From that time the condition remained almost stable. He sat about the ward, always unoccupied, very depressed, never talked to any one except to answer questions. He was distinctly retarded except in the matter of eating, which he did as rapidly as any one. He dressed and undressed himself slowly. He was perfectly oriented as to time and place, and knew the people about him. There seemed to be no memory defect and no thinking disorder. The following is a note made August 29, 1905: "Very little change has been noted in the patient's condition. He is always seen sitting dejected and inactive in the sitting room, and if asked to shake hands he extends his right hand very slowly forward. In answering a question he hesitates for some time, and finally whispers out a reply. This observation applies to his conduct on the ward, where his retardation is marked. About two weeks ago he was taken to the laboratary for four days to be put through a number of tests. One of these tests required him to read aloud, which he did quite audibly on the third day. . . . Other tests were responded to in such a manner as to indicate that he not only understood all that was said to him, but that he could do various things calling for a considerable exercise of his power of attention, recognition and discrimination. Once an experiment was explained to him, he co-operated intelligently, and although with considerable retardation, quite accurately. . . . His memory is good, as is also his orientation as to time and place. Depressive ideas remain, as for example, his usual answer to questions concerning his health: "No better—I shall never be any better." (Hamilton.) This patient took part in the experiments reported in this paper from August to December, 1904, and again in August, 1905. He was discharged to go to another hospital Oct. 8, 1905, in the condition that had been characteristic of him for the past year.

Excited Subjects. C., age 48, a mechanic, of fair education, with a fourth attack of insanity, was admitted as a patient to the hospital August 2, 1904. The first attack (March 8 to May 21, 1901) was a depression. The patient had been a heavy drinker and six years before entrance, in 1901, he had taken the Keeley cure. After that he did not drink until a short time before his admission. In the previous summer C. was very irritable and inclined to worry over small matters. Then he lost interest in his work. Four months before admission he stopped work, and thought his friends were against him, avoided them, became morose and remained at home. His memory at that time was not good, he slept poorly, but there was no marked depression. When admitted (March, 1901) the patient said that he stopped work because he could not attend to it and "got balled up." He had difficulty in thinking. Multiplications that required much thinking were difficult and almost impossible. He said spontaneously that his memory was poor and objectively it was shown to be so. Things which he knew well, *e. g.*, when he came to the hospital, when he last saw the physican, etc., took him a long time to think out. He worried because he could not do his work. During the succeeding two and one-half months he improved rapidly, felt able to take up his work again, and said that his memory was good again. His second attack was an excitement (December 4, 1901, to March 18, 1902). After leaving the hospital in May he began to work, bought another business, worked hard, and showed good judgment in his affairs. In November he became exhilarated, and talkative, but not unduly expansive in his ideas. He also showed a certain forgetfulness. During his stay at the hospital he was decidedly euphoric and exhilarated. He showed a great deal of over-activity. He talked much, decorated his room, and showed a distinct flight of ideas. Everything was said to be "fine;" the hospital "the finest place in the world," etc. He showed no memory defect at that time, but on the contrary had a good grasp on details of both present and past events. He gradually quieted down and became more normal and was discharged "much improved." For four months the patient worked well, but was a trifle irritable from time to time, and in the summer became quite tired. About three months before his third admission he began to have difficulty in applying himself to his work, and drank heavily. Afterwards he could not do his work, became seclusive and slept poorly. Readmitted in December, 1902, he was depressed until May, 1903 (third attack). In this attack he was more depressed than he had been in 1901. He said he was 'done for,' would never amount to anything, others did not want him around, etc. In church he felt that the clergyman was talking at him because he talked of vices, and he asked if he should not get up and confess. He was unoccupied, complained of feeling dull in his head, heavy, and said his mind was weak and that he could not remember things. He said very little. Objectively his memory was not found to be particularly poor, he was oriented in regard to the place and time, and he knew the names of those with whom he came in contact. His calculation ability was poor, and he said it was difficult for him. He improved during the five months of his stay and was discharged in May, 1903. Later he became abnormally irritable and excited and returned to the hospital August 2, 1904. During the interval of over a year he drank frequently and at these times neglected his business. At such times he talked alternately, religiously and profanely. During the six weeks previous to his re-entrance to the hospital he was more talkative, sometimes abusive, and he took no rest and went to meals irregularly. During this period he was arrested twice for acting peculiarly. His manner at the hospital was a jolly one, and his talk showed a "flight." He remained exhilarated as evidenced by his general activity in sports and in his movements and talk. He was apt to become irritable at the slightest provocation. This was his condition during the experiments. In May, 1905, it was noted that he had become a little quieter, but that he made the impression of still being mildly exhilarated. July 29, 1905, the patient was discharged. He had become less exhilarated, but remained active. His conversation was clear and not particularly expansive. Throughout the last attack the patient did not seem to realize that his condition was abnormal. The diagnosis that was made was "Manic-depressive insanity, circular form." For a time, and particularly during the first two attacks, there was considerable doubt about the diagnosis. At first he was considered a case of "General paralysis." This was due largely to the presence of certain physical signs, *e. g.*, no pupillary reaction to light, but good reaction to accommodation. Later the tendo Achillis reflex was noted to be absent, but there were no speech defects, no tremors, no gait disturbances, and the other reflexes were found to be normal. In view of the facts that there is no noticeable dementia, and that there is a regular alternation of the depressions and excitements, it seems most probable that the man is not a case of paresis. If, however, it happens that he is found to be paretic, the results of the experiments would not be affected, because at the time the tests were made he was undoubtedly in an excited, maniacal condition without dementia. The pupillary disturbance, I think is the result of the man's occupation. He has to look alternately at bright lights and to dark objects, and this would tend to reduce, perhaps abolish, the light reflex.

P., was a bright business man, age 48 at the time of his admission to the hospital, May 29, 1904. At this time he was greatly excited and exhilarated, and remained under hospital care until December 5, 1904, when he was discharged on a visit home. A month later he was brought back to the hospital very depressed, after a suicidal attempt. He is now in the hospital (November, 1905). The course of his disease is a typical mania-melancholia, the *manisch-depressive Irresein* of Kraepelin. Three years previous to the present attack P. had bought a business on a friend's advice and when he found later that it was much worse than any one could have expected, he became melancholy. This abnormal depression was, however, only of a short duration, a couple of weeks. Four weeks before entrance to the hospital P. again became 'worked up' over his business, but this time the insanity took on the excited form. Much extra work had been thrown upon him in the business in which he was engaged, and, in addition, he was occupied with the affairs of another concern that he was purchasing. He took a trip to New York in connection with the new business, and there was very erratic. For example, it is said that he bought 500 pocket knives and distributed them among the street gamins. Returning to Massachusetts he wrecked the furnishings in a Pullman coach, was arrested and finally brought here. On entrance he was talkative, excitable, meddled with everything. He seemed to be perfectly clear in regard to his surroundings. He was expansive, euphoric, and quite convinced of his own power and excellency. There was, however, no absurdity in his expansiveness or in his general behavior. He showed insight into his condition, realized that he was excited and irritable, but wanted to go to a summer resort for rest rather than remain in the hospital. There was a marked flight of ideas. He knew the other patients were insane, knew the names of the people, and was oriented as to place and time. He remained in that condition for about five weeks. Then he became more excited, would not keep his clothes on, was violent when an attempt was made to restrain him. In this state he talked of religious matters and sang very often. He believed that he saw worlds made, and that he lived in another world, although he appeared to be perfectly clear in regard to his surroundings, and only once did he not recognize people. After a week of this violent excitement, he became calmer although still much exhilarated. His talk was voluminous and he did not occupy himself beyond writing letters about his business ventures and erotic epistles, all comparable to his talk. About the middle of September the patient showed signs of improvement. He tended to keep to his room more and not to meddle with things, although he was still exhilarated. He began to have some insight into his condition, explaining it as "brain fever." Later he ceased his continuous talking, although he would chatter so long as any one would ask him an occasional question. He gave up writing letters, and read much with enjoyment. Four or five weeks later he was not so active but remained exhilarated. His talk, however, was more hopeful than his financial affairs warranted. It was in this condition that he first came to the laboratory, and the experiments continued until he was discharged in December. In the meantime, he gradually became more natural in actions and talk, and his excitement was noticed only in a great activity in his games and amusements. As has been said, a month later he returned to the hospital depressed and retarded.

During the time of the experiments, therefore, B. and F. were normal subjects, Ed. and Ev. were greatly depressed and retarded, and C. and P. were mildly excited and expansive. The four last cases, possibly with the exception of Ev., showed a considerable improvement during the course of the experiments both in the laboratory and on the ward. Ev. improved greatly in rapidity in the laboratory, as will be shown later, and he talked more freely and louder, but on the ward there was no appreciable improvement. At no time did any of the subjects show any difficulty in thinking and there was in none of them any appreciable (by tests) memory or attention disorder.

All the subjects seemed willing to take part in the experiments; some of them were anxious to show how well they could do. The experiments also gave them a change of scene and occupation, which was probably an agreeable change to them from the monotony of their ordinary life. The results, therefore, are as trustworthy, it seems to me, as could be expected with normal people of the same grade of intelligence.

The series was continued in some cases for a period of fifteen weeks with intervals, but some of the patients were not used for more than half that time. The results from all the subjects are comparable if we take the results in serial weeks as will be done. The degree of retardation in Ev. and Ed. differed, but so far as the general problem is concerned, this variation is not of great importance. C. and P. at the time of the experiments did not greatly differ in the degree of exhilaration, but if there was a difference, P. was probably the more excitable.

Description of Experiments. All the work to be reported in the present paper deals with the time of certain mental processes. A number of other experiments were made on accuracy, memory, judgment, and apprehension, but these will not be published at this time. The object of the time tests was to determine the amount of slowing in the cases of retardation, and any marked increase in ability in the exhilarated patients who were convinced of their own excellency.

The following seven kinds of experiments were made:

A. The time of rapid tapping.
B. The time of the simple reaction to sound.
C. The time of choice reactions to sounds.
D. The rapidity of reading.
E. The time of discriminating and marking out letters.
F. The time of adding.
G. The time of discriminating and distributing colored cards.

In addition to these tests a few other time measurements were made and these fewer and less systematic experiments will be mentioned incidentally in the text. It should be noted that most of the tests were made as simple as was consistent with accuracy, in order that the same kinds of experiments might be performed later on the wards with other patients, who could not conveniently be brought to the laboratory, or who might become unduly excited in a strange situation. With the exception of the reaction time experiments, the tests were of such a character that the patients were tolerably familiar with the materials and methods that were employed.

The results have been grouped by weeks, and the average weekly averages are given in the following tables unless otherwise designated. From these averages the average variations were calculated and these are also given in the tables. The number of experiments in one week varied with the character of the test. In such tests as rapid reading, tapping time, etc., usually five determinations were made each week, but in the reaction time experiments from ten to one hundred were made each day. The numbers of experiments of each kind are noted in the appropriate tables. The arbitrary weekly division of the results was made in order to determine the practice effect.

A. The method for determining maximum rapidity of tapping was as follows: A sheet of paper, 8 x 10 inches, was placed before the subject, a pencil was given to him, and he was instructed (and shown) to tap progressively in lines back and forth on the paper at his maximum speed. The signals for starting and stopping were explained to him, and the experimenter watched the tapping of the subject to see that the beginning and end of the process coincided with the signals. The tapping movement was made by the forearm, although no insistence was made on this point. Most of the subjects remained seated at the table for the experiment, but one (C.) said it was much easier for him to stand up to tap. Thirty seconds was chosen as a convenient time in which the subject had an opportunity of warming up and of attaining his maximum speed, and it seemed not sufficiently long to show any plain evidences of fatigue. In this method there is a possibility of a constant error of plus one tap, but probably not more, and this constant error was probably the same in all the subjects. The number of taps in the thirty seconds was counted, and the time for making one tap was found by dividing the total time by the number of dots on the paper. The differentiation of the parts of the tapping process, *i. e.*, the holding down, the holding up, etc., could not be calculated from the records. Such details were considered unnecessary in this work. Usually only one experiment of this character was made each day.

B. The time of simple reaction to sound was determined by means of a Hipp chronoscope. The patient was instructed to hold down the electric key, and to release it so soon as the sound was heard. The stimulus was produced by an electric telegraph sounder, and was a clear, sharp, rather loud sound. Two or three days' practice in reacting to the sound was given to each subject before any time measurements were made. This was done to familiarize the subject with the apparatus and to get rid of any possible fear of electricity, etc., that might have been present at the beginning. None of the patients showed, however, the least sign of fright, and all reported that they had none. In each day's series about half a dozen practice reactions were made before the time measurements were taken. All the subjects were right-handed, and only the right hand was used in these experiments. The chronoscope was controlled frequently by a fall hammer. In the calculation of the records the weekly averages and the average variations were determined.

Experiments on the reaction to light and tactile stimuli are planned to supplement the present ones on sound, and reactions with the lips as well as with the hand. For a solution of the general problem, *i. e.*, the localization in the nervous system of the retardation and excitation processes such experiments will probably be necessary.

C. The sound apparatus used in the experiments on simple reactions was employed in the choice reaction tests to give a loud sound (same intensity as in simple reactions), and for the less intense sound a telegraph key was hit gently. The intensity and quality of the low sound differed from the other sound, and the intensity probably also varied in the different experiments. The right hand reacted to the sound for the telegraph sounder, as in the simple reactions, and the left reacted to the sound from the telegraph key. The reactions with the right hand were used for comparison with the times of the simple reactions. Only these right hand reactions were timed, but the subjects did not know that the left hand reactions were not being taken. This procedure, *i. e.*, considering only the right hand reactions, was necessary in order to make the results comparable with the results for the simple reactions.

D. The time necessary to read aloud one word was found from a rapid reading of a page of printed matter from three hundred sixty to four hundred words in length. The subjects were given the page of printing and instructed to read aloud at a maximum speed to the end of the page. The total time divided by the number of words gave the average time for reading one word. The matter which was read was unfamiliar to all the subjects. The words, however, were neither peculiar nor difficult. The type was 10-point, single leaded. In experiments such as these the time will vary greatly with the individual, since education and previous practice play great parts in the ease of reading. The patients who were used, however, did not vary very much in their average of intelligence, and possibly not much in their reading practice. It is possible that C. was least intelligent and the least read of all the patients and Ev. the most intelligent and most widely read. These are the impressions the writer got from careful observation of the subjects, but it is extremely difficult, almost impossible, as all know, to make any accurate estimate.

E. One hundred E's in a number of words with an average total of 850 letters were to be discriminated and crossed out as rapidly as possible. The subjects were not informed how many letters there were to be crossed out, but only to do the work accurately and rapidly. This experiment, but with 100 A's in a total of 500 letters, was used in the tests of the Columbia College students.[1] The time for the total task was taken by a stop watch. The number of omissions was noted. The proposed method of calculating the results by lengthening the time proportionately to the number of omitted letters was tried, but was given up. The total time in any one experiment is the sum of the time for discriminating the 850 letters, and the time for marking the 100 E's. In the tables both the total time and the number of omissions are given. This test was very unsatisfactory, owing to the two factors of variability, time, and accuracy. With some subjects the accuracy did not greatly vary, and with others the time was fairly constant.

F. The time of adding was obtained from a series of twenty problems. Each problem consisted of two five-digit figures, one placed over the other, *e. g.*, $\begin{array}{l}43678\\ \underline{34924.}\end{array}$ I had prepared twenty-eight different sheets, each with twenty problems, and each problem differing from the others. These sheets were used in regular order for each of the subjects. The time interval between the first and second use of any particular sheet was so long that there could be no memory of the particular problem. The results of the additions were written by the subjects below the problems, and a check could then be made on the accuracy. The total time, from the start until the last figure in the sum of the twentieth problem was written, was noted. In some cases, particularly Ev. and, at first, Ed., there was considerable hesitation between the problems, and these two subjects had at first to be 'prodded' to proceed to the next problem. There was, accordingly, considerable lost time, and this also was noted. If this lost time is subtracted from the total time we can find the actual time for the addition and the writing of the answers. In the tables in which these results are found there will be noted the designations, 'total time' and 'actual time.' These results are to be understood as explained above. No determinations of the 'actual time' could be made on the writer.

The time lost between the problems could be determined only approximately; the error may be as much as one half second, but probably there were sufficient plus and minus variations in the individual estimations of the nineteen intervals to counterbalance to some extent.

F. The discrimination and marking of the E's proved in so many ways a difficult test to interpret that a few weeks after the beginning of the series on Ed., Ev., and F. I introduced the additional experiment of discrimination and distribution of colored cards. Ten cards each of a different color were placed in a semicircle on the table in front of the subject, and he was given a pack of one hundred colored cards, ten cards of each of the colors represented on the table. He was instructed to distribute the cards as rapidly as possible, and to place the cards of one color in the appropriate place. The cards were so shuffled that no two cards of the same color came together in the pack. Each card was three inches square. The colors I used were: white, light pink, pink, red, yellow, gray green, very light green, light blue, blue, gray. The cards were placed in the foregoing order beginning either at the left or right hand. Sometimes the white, pink, red end of the semicircle was placed at the right, sometimes at the left. No regular order was used and no habit in that respect could be formed. There was sufficient difference in the colors to make them easily distinguished from each other, but the differences between white and light pink, light green and light blue, and gray and green gray, were on the other hand sufficiently small to demand close attention to the distribution. Occasionally a subject would put two cards at one time in the pile, the second card not having been handled and discriminated. This was always counted as one error. More often the mistakes were mistakes in discrimination. The cards which were the most difficult to discriminate were sometimes confused, for example, a gray would be placed on the green gray pile. Sometimes in these piles there would be collections as follows, pink, pink, pink, white, white, white, etc., and white, white, white, pink, pink, etc. Each of these arrangements was counted one mistake, since the discrimination and sorting went on just as if the white and pink positions had been interchanged. The time given in the tables is the time obtained plus a proportionate amount for the errors of omission.

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[1] See Cattell and Farrand: Physical and Mental measurements of the Students of Columbia University, *Psychol. Review* (1896), III; and Wissler: The Correlation of Mental and Physical Tests, *Psychol. Review Monograph Suppl.*, No. 16, 1901, pp. 62.

In the tables the averages are grouped according to the serial weeks, to make the results of all the subjects as comparable as possible. The experiments were not, however, made at the same time. The series on Ed , Ev. and F. were begun August 22-27, B's series, Sept. 5-10, and the series on C. and P. Oct. 23-29, 1904. A few experiments were made on Ev. August 21, 22, 23 and 24, 1905. S., a subject in another series of experiments, but whose results will be considered in the appropriate places, was used July 6-21, August 8-19, and August 21-26, 1905. All the experiments on C., P. and S. were begun on the same day. The experiments on sorting the colored cards, it has been mentioned, were not begun for some time after the other tests with B., Ed., Ev., and F., and the reaction time experiments were not begun until the third week of the tapping, reading, etc., on Ed., Ev., and F.

Experimental Results. Tapping time. The results of the tapping experiments are given in Table I. Here we find that on the whole the two excited subjects show no variation from the normal, but that the retarded patients are much slower than either the normal or excited subjects.[1] B. and F. start

TABLE I.

Weekly averages of tapping time in thousandths of a second. The average variations are given below the averages. The numbers of experiments, when more or less than five, are in parentheses.

Serial	Normal.		Depressed.		Excited.	
weeks.	B.	F.	Ed.	Ev.	C.	P.
1	156.7 2.6 (4)	153.7 5.7 (6)	221.8 9.8	299.3 45.2 (3)	180.5 9.8 (4)	149.0 5.5 (4)
2	153.2 3.4	139.7 1.8 (3)	195.6 23.4	264.2 16.2	——	——
3	160.8 1.9	132.4 8.6	172.2 6.2 (6)	230.7 9.6 (6)	——	——
4	——	127.2 2.1	169.4 3.5	235.0 3.2	184.6 6.7	145.4 2.6
5	——	128.6 4.7	165.4 3.9	231.0 13.6	——	——
6	161.8 3.4 (4)	——	——	——	185.0 2.4	140.2 1.0
8	——	120.4 2.5	164.0 5.6	232.0 4.8	——	——
9	157.0 4.0	——	——	——	——	——
11	——	121.2 1.8	161.6 4.7	224.4 3.9	——	——
15	——	127.0 3.6	——	234.4 5.9	——	——
General average.	157.8 (23)	131.4 (39)	178.4 (36)	240.7 (39)	183.6 (14)	144.6 (14)

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[1] Dresslar (Some influences which affect the rapidity of voluntary movements, *Amer. Jour. Psychol.*, 1892, IV, 514-527) found that it took 37 seconds, to make 300 taps on the first day of his series, *i. e.*, 0.123 sec. for one tap. He does not state whether or not this experiment had been preceded by any practice tests. His general conclusion

with approximately the same speed, but, while F. improved and greatly lessened his time, B. did not gain in speed with practice. Neither C. nor P. show much practice effect. The slight improvement shown by P. is due mainly to one chance result on the third day of the first week, when his time was greatly slowed, to .157 second. On this day it was noted that P. was greatly distractible, and that he kept talking throughout the experiment. If this result be excluded there would be no difference between the results of the first and the fourth weeks. This distractibility was noted throughout the series on both C. and P., particularly at the beginning. The practice effect shown by Ed. is coincident with his general mental improvement, and it is difficult to estimate how much of the increased speed is due to the recovery of the patient and how much to the practice. In this and in the other experiments, as will be shown later, Ev. attained his greatest speed after two weeks' practice, and thereafter the extra practice did not seem to decrease the time for the performance of any of the tests. This is very different to the result found with Ed., and to that obtained from another depressed and retarded subject, S. The results of similar experiments on S. over a period of thirteen weeks are given in Table II.[1] S. shows the gradual increased speed from practice, but in this case, it is also impossible to estimate the amounts due to recovery and to practice. The fact that there is an increased speed from the ninth to the thirteenth weeks, when there were no experiments in the interval, would indicate that a large part of the 'practice effect' was due to the recovery. Similar reductions in time are noted in the figures given by Ed. for the fifth and eighth weeks, and for the eighth and eleventh weeks.

TABLE II.

Weekly averages of tapping time in thousandths of a second. Subject, S., retarded depression.

Serial weeks.	1	2	3	6	7	8	9	13
Averages,	243.3	220.0	204.7	200.7	191.5	190.5	193.0	178.0
Average variations,	14.3	13.5	6.9	7.6	6.2	7.6	1.0	7.0
Numbers of experiments.	4	12	10	10	12	12	2	2

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from his work is that the "normal rate for most rapid voluntary movement of the right wrist was found to average 8.5 taps per second." Other tapping results, but with restricted movements, will be found in an article by Bryan: On the development of voluntary motor ability, *Amer. Jour. Psychol.* (1892), V, 125-204.

[1] S. had been used in another part of this general research, and his

In five experiments Ca., another retarded subject, averaged 162 8 taps in 30 seconds, average time for one tap 0.184 sec. In these five experiments there was noticeable some practice, but owing to mental confusion it was deemed advisable not to continue the work. Pr., a depressed case without retardation but with a feeling of inadequacy,[1] showed considerable speed in movement. Two experiments averaged 196.5 taps in thirty seconds, average time for one tap 0.153 sec. Arranging the subjects in the order of rapidity we have: F., P., Pr., B., Ed., C., Ca., S., and Ev., if all the experiments are grouped, but if the first week's results are considered alone we find the following order: P., Pr., F., B., C., Ca., Ed., S., and Ev. The last four subjects, it will be remembered, were retarded at the time the experiments were made.

The average variations are relatively small, with two exceptions, viz., Ev's first week, and Ed's second week. In both cases the size of the deviation is due to one relatively slow day.

Reaction time. The results of the simple and choice reactions to sounds will be found in Tables III, IV, and V. The experiments on B., C., and P. were begun at the same time as the other tests. Those on Ed. and Ev. were not begun until the third week. Ev. is the only subject to show any great variation from the normal in either simple or choice reactions.

TABLE III.

Weekly averages of simple reactions to sound, in thousandths of a second. The average variations are given below the averages. The numbers of experiments are in parentheses.

Serial	Normal.		Depressed.		Excited.	
weeks.	B.	F.	Ed.	Ev.	C.	P.
1	164.6	157.1	191.3	299.0	182.9	185.0
	15.0	17.9	35.2	79.7	22.7	23.5
	(80)	(140)	(120)	(120)	(240)	(240)
2	167.2	——	181.3	209.2	——	——
	18.7		20.8	53.3		
	(100)		(100)	(100)		
3	165.0	——	178.2	208.3	——	——
	12.2		18.7	41.5		
	(100)		(100)	(100)		

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results will be mentioned in this paper in connection with the appropriate experiments. S., a business man, was 44 years old at the time the experiments were made. He had had two previous attacks of depression with retardation. The attack in which he was the subject of some experiments began in March, 1905, and he was discharged from the hospital in September. A full account of the work on S. will appear in a forthcoming number of the *American Journal of Insanity*.

[1] *I. e.*, a feeling that things are more difficult to do, and a disinclination to do things.

TABLE III. *Concluded.*

Serial weeks.	Normal.		Depressed.		Excited.	
	B.	F.	Ed.	Ev.	C.	F.
4	158.4 14.0 (540)	—	186.1 34.3 (530)	224.0 48.0 (610)	173.7 16.7 (300)	187.5 15.8 (300)
6	159.9 14.3 (80)	—	—	282.9 62.6 (100)	167.9 14.0 (240)	195.2 19.2 (270)
9	163.6 11.0 (300)	—	175.7 19.1 (300)	311.5 57.1 (300)		
13	—	—		342.8 73.6 (380)		

If the average simple reaction time obtained in the tests of college students[1] be taken as the normal average for unpracticed subjects we find the results of B. and F., the two normal subjects used in this work, differ very little from the normal average. The averages of C., Ed., and P. for the first week do not greatly exceed Wissler's determinations plus the probable error (*i. e.*, 158.7+18.9). Ed. always reported that in this experiment his attention was directed to making the movement. If this statement be accepted as evidence of motor reactions, his reactions may be considered slow. It should be noted, however, that when well enough to be discharged his average (ninth week) was 175.7. Another retarded subject, S.,[2] averaged for 350 experiments, 50 on each of seven days, 215.4 (probably sensory reactions). The first set of twenty-five reactions averaged 325.6 and the last set 152.2. This is a decided slowing in the first experiments, and there is a wonderful practice effect in the increased speed of the reaction. The results of the tests of the Columbia students are, however, not directly comparable to the averages for the first week of my subjects. It would be more justifiable to compare the results obtained on the first day from the subjects I used. In this case it should be remembered, as has been noted above, that the subjects were practiced for a time before any time measurements were taken. The results of the first day's simple reaction averages are as follows: B., 164.1; Ed., 144.1; Ev., 446.2; C., 185.9; P., 192.9. The perfectly normal character of Ed's reaction on the first day is remarkable. On the remaining five days of this week, however, this subject averaged respectively 213.0, 189.8, 213.8, 212.0, and 195.7. The reactions on these days are slowed, and are not consistent with the results of the first day. The averages for the excited patients, C. and P., show no shortening of the reaction time, but on the contrary if the difference to the normal is considered sufficient to notice it is a decided slowing. The results which I have obtained should be compared with the results of other investigations. The averages given by Richet[1] for simple reactions to sound are 194 for cases of melancholia and 156 for mania. Bevan Lewis's experiments show a simple reaction to sound in melancholia to be very long, 0.23 second.[2] The average for five maniacal patients was 186. Some of Lewis's patients, that were greatly depressed, gave very rapid reactions, *e. g.*, J. H. B., .13 sec.; C. K., .14 sec.; and J. E., .13 sec. None of his excited patients gave average simple reactions less than .17 sec. In Marie Walitzky's experiments on the reaction time to sound, it was found that in two cases of excitement the simple reaction was .172.[3] v. Tschisch gives the average of 150 simple reactions to sound by a patient recovering from mania 0.07 second,[4] but I feel confident that this time is too short to be considered typical of these cases. No other observer has, to my knowledge, found such a shortening of the time. Janet has also made numerous determinations of the reaction time in depressed conditions.[5] He seems to find in these conditions a slowing of the reactions. The results obtained by Buccola[6] I regret to say I have not seen. The *debile* whose reaction times are given by Pelletier is probably a case of dementia.[1]

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[1] Wissler: The Correlation of Mental and Physical Tests. *Psychological Review Monograph Supplement*, 1901, III, No. 16, pp. 62. Wundt gives 120σ to 180σ as averages of normal subjects, *Physiol. Psychol.*, III, p. 416.

[2] See above for a very brief account of his condition.

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[1] *Dictionnaire de Physiologie*, Vol. III, p. 29. Article *Cerveau*. No definite information is given regarding the cases used in obtaining these results, and it is impossible to draw any comparison with the retarded patients with whom I worked. Richet says that his results are averages of many observers.

[2] W. Bevan Lewis: *Text-book of Mental Diseases*. London, 1899, pp. 164, 364, and 365. The designations of the subjects do not indicate whether or not retardation was present.

[3] Walitzky: Contribution a l'étude des mensurations psychométriques chez les aliénés, *Revue Philos.*, 1889, XXVIII, 583-595. These cases, it should be mentioned, are maniacal conditions in general paralysis.

[4] W. v. Tschisch: Ueber die Zeitdauer der einfachen psychischen Vorgänge bei Geisteskrankheiten, *Neurol. Centralblatt*, 1885, IV, 217.

[5] See particularly *Névroses et idées fixes*, 2 vols., Paris, 1898. Janet's results are given in the form of curves. The printing or the engraving of the cuts is so badly done that it was impossible, even with the aid of a microscope, to determine the figures which he has obtained. The general character of the curves give a fair idea of his results, however. In the case of depression, Bei, it appears that the simple reaction to sound averaged from 200 to 220 (Vol. II, p. 67).

[6] Buccola: La legge del tempo nei fenomeni del pensiero. *Biblioteca scientifica internat.*, Vol. XXXVII. The work done on the insane by this investigator is said to be very good, but I have been unable to get the articles. His work is referred to by Walitzky.

The choice reaction times obtained by other experimenters for normal, depressed and excited students are very varied. In choice experiments similar to those made by me Tischer found on nine subjects an average of 316,[2] with an individual variation from 293 to 357. In Kraepelin's laboratory numerous experiments on the choice reaction times gave averages from 250 to 350. Walitzky found the choice reactions of two excited cases to average 653, although the simple reaction was only 172.[3] v. Tschisch[4] found a corresponding and almost proportionate increase in his case of 'recovering mania.' Lefmann,[5]

TABLE IV.

Weekly averages of choice reaction times in thousandths of a second. The average variations are given below the averages. The numbers of experiments are in parentheses.

Serial weeks.	Normal.		Depressed.		Excited.	
	B.	F.	Ed.	Ev.	C.	P.
1	250.1 36.3 (40)	192.4 28.1 (60)	268.0 53.3 (60)	432.0 82.3 (60)	298.4 60.7 (120)	258.9 41.5 (120)
2	239.4 22.9 (50)	—	251.2 39.8 (50)	306.8 71.8 (50)	—	—
3	234.8 16.2 (50)	—	223.9 20.6 (50)	338.1 75.2 (50)	—	—
4	236.9 29.3 (280)	—	257.2 44.1 (320)	304.5 55.8 (330)	276.9 47.1 (150)	262.1 32.3 (150)
6	230.3 25.6 (40)	—	—	308.5 47.1 (50)	257.4 58.0 (120)	234.0 38.7 (120)
9	236.8 27.9 (180)	—	241.9 35.6 (150)	313.4 48.5 (240)		
13	—	—		357.6 24.7 (210)		

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[1] Pelletier: *Les lois morbides de l'association des idées.* Paris, 1904. See p. 133.

[2] Wundt: *Physiol. Psychol.*, III, 461.

[3] *Op. cit.*

[4] *Op. cit.*

[5] Lefmann: Ueber psychomotorische Störungen in Depressionszuständen, *Psychol. Arbeiten*, 1904, IV, 603-668.

TABLE V.

Differences between weekly averages of choice and simple reaction times in thousandths of a second.

Serial weeks.	Normal.		Depressed.		Excited.	
	B.	F.	Ed.	Ev.	C.	P.
1	85.5	35.3	76.7	133.0	105.5	73.9
2	72.2	——	69.9	97.6	——	——
3	69.8	——	45.7	129.8	——	——
4	78.5	——	71.1	80.5	103.2	74.6
6	70.4	——	——	25.6	89.5	38.8
9	73.2	——	66.2	1.9	——	——
13	——	——	——	14.8	——	——

working in Kraepelin's laboratory, obtained the following averages respectively for six typical cases of depression-retardation (200 choice reactions each, no simple reactions are reported): 325, 371, 406, 469, 474, 685. The average variations for Lefmann's subjects were very large.[1]

F's choice reactions are very short. This is, I think, due to the fact that he knew the conditions of the experiment—that the reactions of the left hand were not being considered. Although an attempt was made to react naturally with both hands, the knowledge of the methods tended to make him pay more attention to the right hand and the loud sound. This was noticeable in that several times he failed to react with the left hand at the proper time. The times given by B., C., Ed., and P. are shorter than those of the majority of Tischer's eleven normal subjects. Ev., however, took a much longer time to differentiate the two sounds and to react properly. The two excited subjects, C. and P., do not show any increased rapidity, and in fact their lowest weekly averages are not so low as the lowest weekly averages of Ed's experiments.

The results of experiments with S. were about the same as with Ed. The general average for seven days was 256.9. On the first day of his series he averaged 450.9, and on the last day 218.8. It should be remarked, however, that at the time the last experiments were made S. was practically well.

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[1] The results of Lefmann's work are mentioned here mainly for the sake of completeness. It seems to me that his methods of experimentation or of calculation must have had considerable errors which do not appear from the description. Only in this way can I understand the results from a maniacal patient, Ba., who in 50 choice reactions had an average of 153 and an average variation of 160. In other respects the article is not good. The author keeps referring to other work for methods, especially to work of Kraepelin or his pupils, and does not give any other indication in his article of what is there attempted. For example, in the article L. does not state what kinds of stimuli were used in the reaction experiments. For this we must refer to a monograph by Kraepelin published twelve years before.

The differences between the simple and choice reaction times are about normal in five of the subjects. In the case of Ev., on certain days the simple reaction time equalled and sometimes exceeded the choice reaction time.[1] It is a striking fact that the choice time in the two excited subjects, who were supposed to have rapid associational processes, is not shorter than the normal choice time or the choice time of Ed.

Considering the reaction times as a whole it is apparent that the excitability of the maniacal patients is not evidenced by an increase in the speed, and that the retardation is not necessarily a decrease in the speed with which a movement is initiated.[2]

The average time of reading one word has been determined by Cattell for himself to be 0.138 second, and Richet[3] has found that he is able to speak, or to think about ten syllables in one second. The results on the six subjects of the present work are given in Table VI. The time taken by my two normal subjects, B. and F., is much longer than that taken by Cattell. P., an excited subject, took about the same time as B.; C., Ed., and Ev. are distinctly slow.[4] As the experiments progressed the normal and depressed subjects improved, but the excited patients did not. The percentages of improvement due to the exercise in this test, *i. e.*, one hundred minus the best weekly average divided by the average for the first week, are as follows: B., 10%; F., 13%; Ed., 37%; and Ev., 18%.

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[1] These results have been considered in some detail in a previous paper. Franz: Anomalous Reaction Times in a case of Manic-depressive Depression. *Psychol. Bulletin*, 1905, II, 225-232. Similar results were obtained in later experiments with S. The condition, therefore, is one which may be more or less characteristic of certain cases of retardation. A full account of the work on S. will be found in the *American Journal of Insanity*, Franz and Hamilton: The effects of exercise upon the retardation in conditions of depression, 1905.

[2] From the results of fatigue experiments Hoch has concluded that the retardation comes principally at the beginning of a movement; On certain studies with the Ergograph, *Journal of Nervous and Mental Diseases*, 1901, XXVIII, 620-628. The present series of experiments do not bear out this conclusion, but I prefer to await further results before making any definite denial on this point. I believe the explanation offered by Hoch for the conditions which he found to be the most plausible one, although it does not fit the results of the present work.

[3] *Dictionnaire*, III, p. 10.

[4] The calculation of a few records on the basis of syllables gave for F. .104 sec. for reading one syllable. The time of the other subjects for reading a syllable may be determined approximately by multiplying the figures in the table by .6.

TABLE VI.

Weekly average time in thousandths of a second for reading one word. The average variations are given below the averages. The numbers of experiments, when more or less than five, are in parentheses.

Serial	Normal.		Depressed.		Excited.	
weeks.	B.	F.	Ed.	Ev.	C.	P
1	276.3 19.9 (4)	198.0 5.0 (2)	545.0 86.5 (4)	394.0 9.0 (2)	329.0 16.0 (4)	266.0 10.0 (4)
2	277.6 14.2	179.8 11.8 (4)	497.2 22.6	323.0 24.4	——	——
3	253.4 22.3	183.5 12.5 (4)	470.0 14.0 (6)	337.8 5.7 (6)	——	——
4	——	186.6 2.5	451.2 13.8	346.2 12.2	342.6 13.9	269.2 18.2
5	——	176.8 7.3	378.8 29.8	321.6 20.5	——	——
6	249.8 9.5 (4)	——	——	——	368.8 28.0	276.8 6.6
8	——	172.0 6.8	347.8 12.6	322.6 24.3	——	——
9	248.6 16.3	——	——	——	——	——
11	——	181.2 9.8	342.0 14.0	329.0 18.0	——	——
15	——	194.4 4.9	——	373.0 10.4	——	——

TABLE VII.

Weekly averages of time in seconds for marking 100 e's. The average variations are given below the averages, and on the third line the average number of mistakes. The numbers of experiments, when more or less than five, are given in parentheses.

Serial	Normal.		Depressed.		Excited.	
weeks.	B.	F.	Ed.	Ev.	C.	P.
1	110.2 13.8 3.2 (6)	96.0 6.7 2.3 (6)	145.2 13.6 7.3 (6)	131.5 32.8 33.2 (6)	135.8 7.4 13.5 (4)	149.2 18.8 14.5 (4)
2	108.8 7.0 1.8	82.6 5.1 0.6	123.6 14.7 5.4	153.6 17.1 18.4	——	——

TABLE VII. *Concluded.*

Serial weeks.	Normal.		Depressed.		Excited.	
	B.	F.	Ed.	Ev.	C.	P.
3	111.0 6.0 2.4	83.4 9.3 0.2	104.6 6.7 5.8	152.0 13.6 16.0	—	—
4	—	—	—	—	132.8 4.6 11.4	143.4 12.1 2.4
6	112.0 7.2 1.8	80.0 8.5 0.3 (4)	108.0 5.2 6.0	154.4 10.3 18.6	130.6 5.5 7.4	131.0 3.6 2.0
9	103.6 8.1 0.2	79.6 5.9 0.0	102.6 6.3 3.4	163.4 16.5 12.0	—	—
13	—	76.0 7.5 0.0 (4)	—	166.8 8.2 14.2	—	—

The discriminating and marking of the one hundred letters in the paragraph of printed matter was at first a very slow process in both the depressed and excited patients. Table VII gives the results of the six subjects. The averages and average variations as well as the average number of errors are given. These experiments were begun at the same time as the reaction experiments, *i. e.*, 3d week for F., Ed., and Ev. As the experiments progressed there was an increase in speed and a decrease in the number of omitted letters for all the subjects except Ev. Practice increased Ev's time but also increased his accuracy These results cannot be directly compared with the results obtained from college students,[1] but in comparison both B. and F. are quite rapid in the performance of the task. Neither C. nor P. are so speedy as the two normal subjects, and P. is slower than the retarded patient, Ev. There is not much difference in time for the first weeks of C. and Ed., C. was a trifle faster, but Ed. was considerably more accurate. Ed. was quicker than B. after the second week, but he did not approach in accuracy to that of B. Ca., another retarded subject, averaged in four experiments 123.5 sec.; A. V., 10.3.; with average omissions 36.5. These results are similar to those of Ev. The time taken by S. for this experiment averaged longer than Ed's time; twelve experiments in one week, average,

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[1] Wissler: *op. cit.*, 270 college students discriminated and marked one hundred A's in a total of six hundred letters in 100.2 seconds with an average of 2.2 errors.

152.7 sec.; A. V., 8.6; average number of omissions, 10.1. In this case practice for one week decreased the number of errors, but not the time.

The results of the adding experiments are given in Tables VIII and IX. The actual time was not determined for F. and no results are given for him in Table IX. P. is the most rapid of all the subjects, probably because a large part of his business was that of bookkeeping. Ed., who had had considerable experience in adding in his business as hotel clerk, was noticeably retarded, particularly during the first two weeks. The total and actual times taken by Ed. were greater at first than any other subject, and C., who had had little experience in this kind of work, may be considered approximately normal, although the time which he took for the work was more than that of the other excited patient or of the two normal subjects.

The actual time gives a more accurate estimate of the rapidity or slowness in the additions than does the total time. The two cases of retardation had to be prodded continually at first to keep on with the work, and C. had a tendency to lie back and to rest between each problem, which tendency had to be overcome. This is noticeable in the time lost, the differences between the total and actual times. B. averaged in lost time about 13 seconds, P. 11 seconds, C. 18 seconds, and Ed. and Ev. about 19 seconds in each test. We find, therefore, the following order of rapidity in the first week: P., F., B., C., Ev., Ed. A striking fact is that Ev., who was so decidedly retarded, much more so than Ed., was much more rapid.

TABLE VIII.

Weekly averages of total time in seconds for adding twenty problems. The average variations are given below the averages. The numbers of experiments, when more or less than five, are given in parentheses.

Serial weeks.	Normal.		Depressed.		Excited.	
	B.	F.	Ed.	Ev.	C.	P.
1	127.5 6.8 (4)	106.5 11.5 (4)	312.2 87.4	214.0 28.0 (2)	183.5 4.0 (4)	90.8 3.5 (4)
2	103.7 1.5	—	212.6 25.9	198.4 29.9	—	—
3	111.6 4.7	93.8 5.6 (3)	177.2 11.8 (6)	162.5 6.5 (6)	—	—
4	—	92.2 3.5	157.6 8.7	157.8 9.6	171.4 3.7	85.8 1.8
5	—	88.0 1.6	140.4 10.3	159.8 5.8	—	—

TABLE VIII. *Concluded.*

Serial weeks.	Normal.		Depressed.		Excited.	
	B.	F.	Ed.	Ev.	C.	P.
6	109.5 1.5 (4)	—	—	—	164.2 4.7	83.2 1.8
8	—	89.8 4.8 (4)	141.4 13.1	162.2 10.6	—	—
9	102.4 0.7	—	—	—	—	—
11	—	85.2 3.0	126.4 6.9	153.8 6.6	—	—
15	—	82.4 4.5	—	161.8 10.6	—	—

TABLE IX.

Weekly averages of actual time in seconds for adding twenty problems. The average variations are given below the averages. The numbers of experiments, when more or less than five, are given in parentheses.

Serial weeks.	Normal.	Depressed.		Excited.	
	B.	Ed.	Ev.	C.	P.
1	112.6 5.9 (4)	279.6 84.3	191.5 24.5 (2)	161.6 7.4 (4)	78.3 4.5 (4)
2	93.3 2.5	177.8 21.0	172.6 27.9	—	—
3	100.0 5.4	152.3 10.3 (6)	142.1 5.4 (6)	—	—
4	—	141.0 6.8	143.2 8.8	154.6 3.5	75.9 1.9
5	—	127.5 9.0	146.2 5.2	—	—
6	95.4 3.9 (4)	—	—	148.8 5.8	72.9 3.1
8	—	124.5 9.4	142.8 10.4	—	—
9	88.5 0.4	—	—	—	—
11	—	111.4 5.7	137.0 5.0	—	—
15	—	—	142.6 11.5	—	—

The practice improvement is most marked in the case of Ed., a result which has been found also in the previous experiments. But an increase in speed does not follow an increase in the number of experiments. Very soon, in the third week, Ev. attains his maximum speed and thereafter there is no improvement. All the other subjects improve to the end of their series, and this improvement is marked even when rest intervals of two or three weeks are taken. The greatest improvement is found for Ed. He has much more to be improved. Here again it is difficult to determine how much the increased speed is due to the practice and how much to his recovery. Much of the 'practice effect' is undoubtedly due to the improvement in the mental condition. The greatest improvement in the other subjects is only 25 per cent., *i. e.*, Ev., and in B. and F., with about the same number of experiments the results of the last week show only 22 per cent. gain in speed. In Ed's experiments the times of the last week are only forty per cent. as long as those of the first week, an improvement of sixty per cent.

In the fourth week of the series on C. and P., the ninth week on B., and the eleventh week of the work on Ed., Ev., and F., I had all the subjects count from one to one hundred, and to add as rapidly as possible one hundred two-digit problems. One hundred problems, *e. g.*, $\begin{array}{c}2\\ \underline{9}\end{array}$ $\begin{array}{c}6\\ \underline{3}\end{array}$ $\begin{array}{c}4\\ \underline{8}\end{array}$, were placed on a sheet of cardboard and instructions given to add the individual problems and to speak the results as rapidly as possible. After this I had each subject count at a maximum speed from one to one hundred. The times were taken by a stop watch. The results of the experiments are given in Table X.

TABLE X.

Average time in seconds for adding one hundred two-figure problems, and for counting from one to one hundred. The average variations are given below the averages. The numbers of experiments are in parentheses.

	Normal.		Retarded.		Excited.	
	B.	F.	Ed.	Ev.	C.	P.
100 Problems,	85.0 25.0 (5)	69.1 3.6 (7)	78.8 6.3 (4)	92.3 8.5 (4)	144.0 0.0 (2)	70.5 3.5 (2)
Counting,	37.8 0.8 (4)	22.4 1.7 (5)	31.5 2.3 (4)	62.0 4.0 (4)	51.0 0.5 (2)	34.0 0.0 (2)
Difference,	37.2	46.7	47.3	30.3	93.0	36.5

At the time these experiments were made Ed. was not appreciably retarded, *i. e.*, he was not retarded to a degree that could be determined by observation or by any rough tests. His rapidity in this experiment showed, moreover, that he was not retarded in comparison with the other subjects. C. and P. at this time were both excited.

It is interesting to note that the general time relations between the subjects of the same class are the same for these simple additions and for the more complicated addition problems on the same days. Thus, for example, Ed. took 81% as long as Ev. for the five-digit problems and 85% for the one-digit problems. F. took 80% as long as B. for the one digit and 83% for the five-digit problems, and P. in both cases approximated 50% of the time needed by C.

In the comparison of the figures given in Tables VIII, IX, and X, we are justified, I think, in considering that a large part of the time taken by Ed. at first for the five-digit problems was due to the retardation, and that much of his improvement was due to the recovery. Part of the improvement undoubtedly was due to practice. P. continued to show considerable speed in addition, and C. was consistently slow. The time of counting for P. was not proportionately so fast, but C's counting time was slow. Ev. also had slow counting time. Considering the time alone we might say that both C. and Ev. showed a retardation, but it was apparent that the slowing of C. was due rather to an awkwardness, and that Ev's was due to some retarding influence. C. showed throughout the experiments, as is indicated in all the tables, a decided mental and physical clumsiness which was not at all characteristic of Ev. The long average time taken by B. for the one hundred problems was due to the result of one experiment, in which some event must have occurred to greatly lengthen the time. On one day B. took 105 seconds for the test. If the other results be considered apart from this, we find an average time, 58.8 seconds, A. V., 3.0.

The difference in time between the addition and the counting may be taken as approximately the time of the mental operation of adding. This difference is large for C. and small for Ev. We also see that the results of the other subjects agree with each other quite well, and that there is a correspondence between these differences and the choice time (choice reaction time minus simple reaction time).

Ed. shows no retardation, in adding the one hundred problems, in counting, or in the adding time. Ev. is slow in adding and counting but the adding time is rapid. C. is slow in counting and much slower in adding, and his adding time is very slow. P. shows nothing abnormal.[1]

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[1] The averages for this experiment illustrate well the danger of drawing conclusions too hastily from time measurements of mental phenomena. The most evident conclusions from these results alone would be that C. was retarded at the time the experiments were made.

In the discrimination and distribution of one hundred colored cards results were obtained (Table XI) similar to some already discussed. Ev. was very slow, C. was slow, P. was medium in rapidity, and B., Ed., and F. were rapid, increasing in the order named. All the subjects improved from practice.

TABLE XI.

Weekly average time in seconds for discriminating and distributing one hundred colored cards. The average variations are given below the averages. The numbers of experiments, when more or less than five, are in parentheses.

Serial	Normal.		Depressed.		Excited.	
weeks.	B.	F.	Ed.	Ev.	C.	P.
1	157.5 7.5 (2)	119.8 3.3 (4)	151.0 20.8	422.6 30.1	201.0 21.3 (4)	190.0 4.5 (4)
2	133.8 7.8 (4)	128.0 0.0 (1)	—	—	—	—
3	141.5 14.5 (2)	—	—	—	—	—
4	108.8 3.1 (6)	—	—	—	188.2 7.8	134.8 5.9
5	105.2 2.6	109.0 4.7 (3)	120.2 7.4	370.8 15.8	—	—
6	—	—	—	—	171.2 3.4	122.4 4.1
9	—	112.0 4.0 (2)	—	376.8 25.4	—	—

Conclusions. When the results of all the experiments are considered it is evident that the excited patients do not show any consistent increase in speed over the normal or depressed patients. The maniacal condition is, therefore, not an increased motor ability but merely an increased motor diffusion.

The retarded subjects, on the other hand, were slow at the beginning of all the series, but this retardation in the time of mental processes is not regular.

For the performance of what we may call the more complex mental processes, *e. g.*, choice reactions, adding, etc., the retarded subjects do not take proportionately so long a time as they do for simpler acts. The maniacal patients tend to keep the normal relations.

In the retarded patients considerable practice effect was found. This was more noticeable during the first few experiments. In other work I have shown that general exercise increased the speed of some mental processes in a case of retardation, and at the same time lowered the thresholds for pain and touch sensations. These facts indicate that this class of patients may be improved to some extent by systematic exercise, *i. e.*, their movements may be made more rapid. This would not cure the depression but would help by lessening the retardation. In every person there is a tendency for the formation of habits, and not the least in the insane. It is probable that in many cases there is formed the habit of slowness, and this may be supplanted by an activity habit formed by exercise.

From the results of the simple reaction and the tapping experiments it seems unlikely that the retardation comes principally at the beginning of the movement, as has been suggested. If the retardation was a slowing in starting we should expect to have a definite time added to the normal time for all psycho-motor activities, and otherwise to keep the normal time relations (minus this time) for all mental processes. More detailed experiments are needed on this subject.

I have previously suggested that the retardation may be a general lowering of the irritability, but from the experiments already made it is not fully settled where the (supposed) lowered irritability is. Some experiments which I have made show that in cases of retardation the tendon reflexes are slower than normal and that the skin sensibility is dulled. In conjunction with the fact that the addition of extra mental processes does not greatly increase the total time, these facts would indicate that if there is a lowering of the irritability such lowered irritability is not principally in the brain but rather in the peripheral parts of the body, particularly the nervous system. I expect to take up this matter in more detail in another paper.

In only a few cases are the average variations for the insane subjects greater than normal.

ACQUISITION OF WRITTEN LANGUAGE BY PRIMITIVE PEOPLES.

By **Alexander F. Chamberlain.**

The following notes are offered as a brief contribution to the psychology of language-learning from the field of American linguistics. The literature concerning the acquisition of written language by primitive peoples is very limited in extent, and the author has endeavored to bring together here what is known of the attempts by missionaries and others to enable certain American Indian peoples to read and write their own tongues. The remarkable ease with which this has been done in some cases is an important fact, psychologically and pedagogically.

I. *Algonkian*. The first attempt to make the Algonkian Indians acquire an Indian or a European language by means of a specially prepared syllabic alphabet was that of Père LeClercq, a Recollect missionary among the natives of Gaspé for many years, beginning with 1655. Following is his own account of the origin and development of these characters, as given by Shea:

"The easy method which I found for teaching our Gaspesians their prayers with certain characters which I formed, effectually convinces me that the majority would soon become instructed; for, indeed, I should find no more difficulty in teaching them to read than to pray to God by my papers, in which each arbitrary letter signifies a particular word, and some even two together. They so readily grasp this kind of reading that they learn in a single day what they would never have been able to retain in a whole week without the aid of these cards, which they call *kignamotinoer* or *kateguenne*. They preserve these instructive papers so carefully and prize them so highly that they keep them very neatly in little bark cases adorned with wampum, beads and porcupine quills. . . . . Our Lord inspired me with this method the second year of my mission, when, being greatly embarrassed as to the mode in which I should teach the Indians to pray, I noticed some children making marks on birch-bark with coal, and they pointed to them with their fingers at every word of the prayer which they pronounced. This made me think that, by giving them some form which would aid their memory by fixed characters, I should advance much more rapidly than by teaching on the plan of making them repeat over what I said. I was charmed to know that I was not deceived, and that these characters which I had traced on paper produced all the effects I desired, so that, in a few days, they learned all their prayers without difficulty. . . . . I enlarged them so as to include all the prayers of the Church, with the sacred mysteries of the Trinity, Incarnation, Baptism, Penance, and the Redemption."

In his *Algonquian Bibliography* Pilling gives, from LeClercq, a facsimile of the Lord's Prayer in these Micmac "hieroglyphics" (p. 304). It will be seen that some of them are quite complicated. These characters continued in use among the Micmacs, who, as late as 1881, according to Dr. J. G. Shea, could "write and read them."

In 1866, Rev. C. Kauder, a Redemptorist priest, who had gone over to the Trappists at Tracadie, N. B., where he studied the Micmacs and their hieroglyphics, published, with the title, *Buch das gut, enthaltend den Katechismus, Betrachtung, Gesang* (Wien, 1866), a catechism, hymn-book, and book of devotion in Micmac "hieroglyphs" with interlinear wording in German. In this work he is said to have been aided by Michael Christmas, an educated Indian of Nova Scotia. A facsimile of the title-page of this book is given by Pilling (p. 274). Cuoq, in his *Lexique de la langue Iroquoise* (Montréal, 1882) writes of the work of Kauder as "fort beau et très-ingénieux, mais aussi très-dispendieux et, à mon avis, fort peu pratique." But the Abbé Cuoq was no friend of "new graphic systems." Micmac MSS. exist in these "hieroglyphs," but since Kauder's book, none seem to have been printed, at least to any extent. For Kauder a special font of type was cast in Vienna. It is very interesting that, so early as the middle of the seventeenth century, an attempt should have been made to give these Indians a reading knowledge of language.

About 1840, Rev. James Evans, for many years a Methodist missionary among the Cree and other Indian tribes of the Hudson Bay region centering about Norway House, bethought himself of applying a phonetic system for translations into the Cree language, developing by 1841 what is now called the "Cree Syllabary," or "Evans's syllabary." The idea seems to have come to him from some of the shorthand systems exploited about this time in England, as may be seen from comparing his characters with those, *e. g.*, of Pitman, etc. In 1841 Evans began to teach his system to the Cree Indians, and soon set about printing books. The very earliest types were whittled from blocks of wood with a pocket knife; the ink was made from soot; the "paper" used was birch-bark. Some of the original type and birch-bark books are still preserved.

Having been for some years refused permission by the Hudson Bay Company to import press and types, he "cast leaden blocks from the lining of the chests in which tea was brought into the country, and whittled them into shape as best he could, and, by a rough improvised press of his own manufacture, succeeded in printing many hymns, sections of the Holy Scriptures, and primary school-books, which were of great service." A set of these home-made types was sent to England, and soon the Hudson Bay Company gave their consent, "to have a font cast and, with a press, sent out to Norway House, pledges being given that they would be used only for mission work." Evans himself did not live long to carry on his work, dying in November, 1846, after returning to England. His labors have been continued by others, however, and to-day, "the different sub-tribes included in the Cree conferacy are supplied with native literature to a greater extent than any of the other Indian tribes in Canada (Maclean, Canad. Sav. Folk, p. 283)," and "few Cree Indians can be found who are not able to read the literature printed in the syllabic characters (p. 85)." The success of this syllabary was early reported by Ballantyne, in his *Hudson Bay* (Lond., 1848), where we read concerning Evans at Rossville (cited by Maclean):

"I spent a pleasant afternoon in sauntering about the village, and in admiring the rapidity and ease with which the Indian children could read and write the Indian language by means of a syllabic alphabet invented by their clergyman. The same gentleman afterwards made a set of leaden types with no other instrument than a pen-knife, and printed a great many hymns in the Indian language (p. 159)." Rev. John Maclean, in his book on *The Indians* (Toronto, 1889), says of Evans's syllabary:

"It is so simple in construction that an Indian with average intelligence can memorize the whole in a day, and in less than one week read fluently any book written upon this plan (p. 256)." Many of the Indians have learned to read fluently "with no other teachers but the Indians around the camp-fires."

The northern Crees, among whom Evans labored, have naturally taken to it most, but later on it obtained a footing among the Plains Crees and the Stonies, who, according to Maclean, "read the books printed in this system fluently, and write letters in it, some of which I have in my possession." By Rev. E. J. Peck the Evans syllabary has been adapted to Eskimo, and by Father Morice to the Carrier language of the Athapascan stock. Protestant and Catholic missionaries use it, and the Indians of several tribes write it on birch-bark, etc. The total of the literature extant among all the tribes employing the Evans syllabary or modifications of it is quite considerable, as revealed, *e. g.*, by the titles in Pilling's bibliographies, and it has certainly "exerted a great educating influence over the minds of the people." Maclean remarks:

"By means of this syllabary a clever Indian can memorize in an hour or two all the characters, and in two or three days read the Bible or any other book in his own language." Pilling, who calls the Cree syllabary "a great improvement on the Cherokee," says of these characters: "Those who use them in teaching say it takes the average child not two-and-a-half years (as with us) to read fluently, but a few weeks."

II. *Athapascan.* Rev. A. G. Morice, since 1885 a Catholic (O. M. I.) missionary among the Carrier Indians of Stuart's Lake, B. C., published in 1890 *The New Methodical, Easy and Complete Déné Syllabary* (2 pp.), which is reproduced by Pilling in his *Athapascan Bibliography* at pages 67-69. The syllabary is also to be found in Morice's article on *The Déné Languages* (Trans. Canad. Inst., Vol. I, 1889-1890, p. 175). In these characters Father Morice has published a Déné primer, catechism, etc. In illustration of the practical worth of his new syllabary he tells us, "Through it Indians of common intelligence have learnt to read in one week's leisurely study before they had any primer or printed matter of any kind to help them on. We even know of a young man who performed the feat in the space of two evenings."

More recently (Bull. Soc. Neuchât. de Géogr., Vol. XV, 1904, p. 74), Father Morice is on record as saying, in connection with a letter written by an Indian in these characters with a bit of charcoal on a piece of pine-bark: "Our Indians read and write their language, with marvellous facility, by means of recently invented syllabic characters, which they learn without having any regular schooling."

Morice's syllabary, perfected in 1889, is based upon Evans's Cree syllabary, with modifications necessary to express, "the more numerous and delicate" sounds of the Carrier tongue. In an earlier article (Proc. Canad. Inst., Vol. VII, 3d Ser., 1889, p. 166), Father Morice wrote:

"I am now continually in receipt of letters from Indians whom I never taught and who have learned to read after one or two weeks (in some cases I might say three or four days) private instruction from others."

Rev. W. W. Kirkby, for a number of years (1870-1879) an Episcopal missionary among the Chippewyan Indians of the Athapascan stock, about the Churchill River, published several translations (hymns and prayers, manuals of devotion, Gospels of St. John and St. Mark, New Testament, portions of the Book of Common Prayer, etc.) into the Chippewyan tongue in a syllabary based upon, or rather identical with, that of

Evans for the Cree, etc. Religious publications in this syllabary have appeared in other Athapascan languages, *e. g.*, the Slavé. The syllabary publications of Perrault (1857–1865) and Legoff (1890) in Chippewyan (Mountaineer) belong here also.

III. *Chinookan.* Father Le Jeune, a missionary priest (O. M. I.) since 1880 among the Thompson, Okanagan and Shushwap Indians of British Columbia, has adapted for use in stenographic and mimeographed publications (vocabulary, hymns, primer, etc.), in the Chinook jargon, still a *lingua franca* of this region, the Duployan system of short-hand, which made its appearance in Paris in 1867, Father Le Jeune (he was then 16) learned it in a few hours. In July, 1890, he thought of trying it "as an easy phonetic writing for the Indians of British Columbia," first on the Nicola, then in succession on the Shushwap and Thompson tribes. In May, 1891, he began to publish the Kamloops Wawa, a periodical in the Chinook jargon in stenographic characters and mimeographed copies. A facsimile of the first page of the first number is given by Pilling in his *Chinookan Bibliography*.

IV. *Eskimoan.* The Evans characters have been introduced among some of the Eskimo communities. Rev. E. J. Peck, an Episcopal missionary, the scene of whose labors has been successively Little Whale River in the Ungava district and (since 1894) Blackhead Island in Cumberland Sound, translated into the Eskimo language of those regions certain portions of the Scriptures and had them printed in the syllabic characters of Evans. He then began at once to teach the Eskimo to read. His experience was that the natives of both sexes and all ages learned to read their language with wonderful facility. Of the 40 families, whose headquarters were at Little Whale River, it is related that after a little more than two years had elapsed, "they can all, with few exceptions, read their books." Indeed, there are instances on record in "which a couple of weeks' instruction sufficed to enable a native to decipher texts." A copy of the Eskimo syllabary is given by Pilling in his *Eskimoan Bibliography* (p. 73) and by Maclean in his *Canadian Savage Folk* (Toronto, 1896, p. 504).

V. *Iroquoian.* The Cherokee branch of the Iroquoian linguistic stock has been distinguished by the invention of the so-called "Cherokee alphabet" properly a syllabary, the device of Sequoyah, or George Guess (or Gist), whose father was probably a "Pennsylvania Dutchman," his mother a Cherokee squaw. Pilling writes of him:

"An illiterate vagabond, vague and dreamy, if report be true, who could read neither his own nor any language, and was taunted, it is said, with this fact by some white men;

whereupon, so the story goes, he retorted that he would learn and teach his brethren as well." Foster, in his biography, styles him "Sequoyah, the American Cadmus and Modern Moses . . . . the greatest of all Red Men, around whose wonderful life has been woven the manners, customs and beliefs of the early Cherokees, together with a recital of their wrongs and wonderful progress towards civilization." As he was born *ca.* 1760, Sequoyah, whose "dreamy meditations on this invention" are placed in 1809-1821, must have been much past middle age when it was fully achieved. There is no evidence that he attended school as a boy, for the first mission among the Cherokee was not established until long after he had reached manhood. The earliest known account of the "Cherokee alphabet" dates from 1826 (cited by Pilling from *Missionary Herald*, Feb., 1826, pp. 47-49):

"A form of alphabetic writing, invented by a Cherokee named George Guess, who does not speak English, and was never taught to read English books, is attracting great notice among the people generally. Having become acquainted with the principle of the alphabet, viz., that marks can be made the symbol of sound, this uninstructed man conceived the notion that he could express all the syllables in the Cherokee language by separate marks or characters. On collecting all the syllables which, after long study and trial, he could recall to his memory, he found the number 82. In order to express these he took the letters of our alphabet for a part of them, and various modifications of our letters, with some characters of his own invention, for the rest. With these symbols he set about writing letters; and very soon a correspondence was actually maintained between the Cherokees of Wills Valley and their countrymen beyond the Mississipi, 500 miles apart. This was done by individuals who could not speak English, and who had never learned any alphabet but this syllabic one, which Guess had invented, taught to others, and introduced into practice. The interest in this matter has been increasing for the last two years, till, at length, young Cherokees travel a great distance to be instructed in this easy method of writing and reading. In three days they are able to commence letter-writing, and return home to their native villages, prepared to teach others. . . . . . Either Guess himself, or some other person, has discovered four other syllables, making all the known syllables of the Cherokee language 86. This is a very curious fact, especially when it is considered that the language is very copious on some subjects, a single verb undergoing some thousands of inflections."

In 1827, Rev. S. A. Worcester compared the "Cherokee alphabet" with the alphabet of Pickering as follows:

"I am not insensible of the advantage which Mr. Pickering's alphabet, in common with that in use at the Sandwich Islands, possesses above the English, by being so much more nearly a perfect alphabet. Nor do I suppose that more than half the time would be required for a Cherokee child to learn to read his own language in that alphabet, which is required for an English child to learn his. But, in point of simplicity, Guess has still the pre-eminence; and, in no language, probably, can the art of reading be acquired with nearly the same facility."

In 1827 a font of type, specially cast for the new syllabary, and a printing press were sent from Boston, and a national paper in the Cherokee language and alphabet, the *Cherokee Phenix*, was established in February, 1828, but lasted only six years. After the removal of the Cherokee to the Indian Territory, there was begun, in 1844, the *Cherokee Advocate*, of which Mooney informs us (pp. 109-113; 219-220):

"It is still continued under the auspices of the Nation, printed in both languages and distributed free at the expense of the Nation to those unable to read English,—an example without parallel in any other government." Besides being employed in a vast amount of religious literature (Bible translations and extracts, hymn-books, etc.), newspapers and periodicals, text-books and school-books, law-codes, etc., political and other tracts, and the like, "the syllabary is in constant and daily use among the non-English-speaking element, both in Indian Territory and in North Carolina, for letter-writing, council-records, personal memoranda, etc." Mr. Mooney calls attention also to another use of the syllabary:

"What is perhaps strangest of all in this literary evolution is the fact that the same invention has been seized by the priests and conjurers of the conservative party for the purpose of preserving to their successors the ancient rituals and secret knowledge of the tribe, whole volumes of such occult literature in manuscript having been obtained among them by the author." Some of this literature has been published by Mr. Mooney, the rest lies in the library of the Bureau of American Ethnology at Washington. The effect of the "Cherokee alphabet" is described by Mr. Mooney.

"The invention of the alphabet had an immediate and wonderful effect on Cherokee development. On account of the remarkable adaptation of the syllabary to the language, it was only necessary to learn the characters to be able to read at once. No schoolhouses were built and no teachers hired, but the whole Nation became an academy for the study of the system, until, in the course of a few months, without school or expense of time or money, the Cherokee were able to read and write in their own language. An active correspondence began to be carried on between the Eastern and Western divisions, and plans were made for a national press, with a national library and museum to be established at the capital, New Echota. The missionaries, who had at first opposed the new alphabet, on the ground of its Indian origin, now saw the advisability of using it to further their own work."

As early as 1823 the Cherokee National Council awarded Sequoyah "a silver medal with a commemorative inscription in both languages," and the treaty of 1828, made in Washington, stipulated for the payment to him of $500, "for the great benefits he has conferred upon the Cherokee people, in the beneficial results which they are now experiencing from the use of the alphabet discovered by him."

Of the "alphabet" itself, worse even from a phonetic than from a mechanical point of view, Pilling observes (p. 183):

"This syllabary is one of the most curious compounds imaginable,—worse, perhaps, than would be expected to come from even such a source. Based, as I have said, upon the Roman characters found in the spelling-book, he took all sorts of liberties with them, subjected them to all kinds of indignities, turned them upside down, wrong side to, added tails when fancy dictated, and sometimes even horns. They are hard to make, cannot be joined together, as in our script, and altogether constitute as varied a hodge-podge as ever the untutor'd mind could desire. As characters they possess but one redeeming trait,—once memorized, it would scarcely be possible to forget them." In spite of all this, however, "a few hours of instruction are sufficient for a Cherokee to learn to read his own language intelligibly," and in two and one-half months the Cherokee child "acquires the art of reading and writing fluently in these rude characters."

The centennial of the invention of the "Cherokee alphabet" may well be celebrated by white men and red men alike.

VI. *Salishan*. As noted above, when discussing the Chinookan, the Duployan system of shorthand has been adopted by Father Le Jeune for translations (prayers, hymns, catechism, primer, etc.) in stenographic characters reproduced by the mimeograph in the Thompson, Shushwap and Okanagan languages of the Salishan stock. After trying, with no success, to teach the Indians to read in English characters, the thought came to him, in 1890, to try the shorthand system which he had learned as a youth. The account of its inception and progress, as given by Father Le Jeune, is as follows (Pilling, *Chinookan Bibliography*, p. 48):

"The first trial became a success. At the end of September, 1890, a poor Indian cripple, named Charley Alexis Mayoos, from the lower Nicola, saw the writing for the first time, and got the intuition of the system at first sight. He set to decipher a few passages of Indian prayers in shorthand. In less than two months he learned every word of them, and he soon began to communicate his learning to his friends and relatives. Through his endeavors some eight or ten Indians at Coldwater, Nicola, B. C., became thoroughly acquainted with the writing system before April 1, 1891. In July, 1891, the first lessons were given to the Shushwap Indians; they lasted an hour every day for four or five days. Three or four of the best young men went on studying what they had learned, and were delighted to find themselves able to correspond in shorthand in the early fall. During the winter months they helped to propagate the system of writing among their people. In the meantime Mayoos had come to Kamloops and was pushing the work ahead among the young people there. In December, 1891, the system was introduced to the North Thompson Indians; in January, 1892, to those at Douglas Lake; in February, at Spuzzum and North Bend; and last of all, in March, to those at Deadman's Creek, near Sarvina. Soon after Indian letters came from William's Lake. In May, 1892, a few lessons were given at St. Mary's mission to the Lower Fraser and sea-coast Indians. Now the Indians teach each other and are very anxious to learn on all sides. The most advanced understand the value of the letters and the spelling of the words; but the greatest number begin by reading the words, then learn the syllables by comparing the words together, and at last come to the letters. They learn by analysis and much quicker than by synthesis."

A facsimile of a page of prayers in the language of the Thompson Indian is given by Pilling in his *Salishan Bibliography* (p. 40), and by Maclean, in his *Canadian Savage Folk* (p. 539).

VII. *Siouan.* According to Rev. John Maclean, the Canadian Stories of the Siouan stock, "read the Evans syllabic characters, and write them freely and neatly." Lord Southesk, in his *Saskatchewan and Rocky Mountains* (London, 1875) writes, —the real date is 1859-1860,—thus (p. 250):

"Our Stony messenger met us on the road, bringing me a letter from his people written in the Cree syllabic characters."

This letter from the Mountain Assiniboines (Stonies) is reproduced in this work.

In 1884-1885, a Winnebago (Siouan stock) Indian of Nebraska, on a visit to the Sac and Fox (Algonkian) acquired an "alphabet" in use among these people, which has been described by Miss Alice C. Fletcher in the modified form in which it became current among the Winnebagos. Writing in 1890, Miss Fletcher says:

"He taught others of his tribe, and the knowledge spread rapidly among the Winnebagos of Nebraska, and also to that part of the tribe living in Wisconsin, so that, at the present time, the principal correspondence of the tribe takes place by means of these characters."

In August, 1885, the Indian Agent wrote to Miss Fletcher:

"The tribe have suddenly taken to writing their own language, and people who have never learned English have acquired this art. The people claim they took the basis of it from the Sauk and elaborated it themselves. It is a very suggestive sight to see half a dozen fellows in a group, with their heads together, working out a letter in these new characters; it illustrates the surprising facility with which they acquire what they want to learn."

The Winnebago "alphabet" is really compounded of alphabet and syllabary on the basis of English letters. There are 15 initial sounds and four vowel modifiers, forming 128 combinations like syllables; and with these "one can easily write any word in the Winnebago language." There is a certain originality about it, for Miss Fletcher tells us:

"I have examined the Cherokee alphabet, thinking this one might be an outgrowth or corruption of that invented by Sequoyah, but it does not seem probable to me."

She remarks further:

"The education of Indian youths in English has set Indians to thinking of how they can preserve their language, and I have seen many boys and girls who have labored to make our English letters bend about the Indian words. It would seem as though we might in time expect several such inventions as this chart, but they will all probably have the same fate as our own childish devices to create a new language and a new alphabet."

Conclusions. 1. The data recorded above describe efforts by white men or by Indians to enable American aborigines belonging to seven different linguistic stocks of North America to read and write by means of some kind of syllabary or phonetic "alphabet." The success achieved is often striking and the ease displayed in learning remarkable,—indeed a widespread facility in this direction is clearly indicated, particularly in the case of children and youth (the skill in acquisition by adults is also noteworthy). These facts are of considerable importance in connection with the problems involved in the acquisition of reading and writing by our own children.

2. The use of these "alphabets" varies from employment for the narrower purposes of the missionary, and his religious or ecclesiastical needs among some of the northern tribes to a national utilization such as is in vogue among the civilized

Cherokee and their heathen brethren of the Carolinian mountains. The total amount of literature of all sorts printed is quite large and is increasing in bulk.

3. The influence upon the Indian tribes of the acquisition of the ability to read and write their own language has been good and often led to the preservation of interesting material that would otherwise have perished altogether. The result of the invention of the "Cherokee alphabet" upon that Indian people is a reality that might well be compared with the fabled achievements of Cadmus in the ancient world.

4. The sources of these "alphabets" and the inspiration which gave them birth are diverse. Father Leclercq got his idea from observing the Indian children making marks on birch-bark as an *aide-mémoire* to their prayers; Evans's and Le Jeune's systems are evidently due to stenographic prototypes; Sequoyah was stimulated by an English spelling-book.

5. Altogether the history of the acquisition of written language by the primitive peoples under discussion forms an interesting chapter in racial pedagogy and psychology.

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20. **Worcester**, S. A. Invention of the Cherokee Alphabet. *Ibid.*, pp. 330-332.

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Probably the earliest printed item in the "Cherokee alphabet."

AN EXPERIMENTAL EXAMINATION OF THE PHENOMENA USUALLY ATTRIBUTED TO FLUCTUATION OF ATTENTION.[1]

By C. E. FERREE, A. M., M. S.

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[1] From the Psychological Laboratory of Cornell University.

INTRODUCTION.

It is commonly admitted by experimental psychologists that no final explanation has as yet been offered of those fluctuations of minimal stimuli and minimal stimulus-differences which go by the general name of 'fluctuations of attention.' We have peripheral theories and central theories and mixed, peripheral-central theories; and, without doubt, we have a good deal of scattered knowledge about the conditions which underlie the phenomena in certain of the fields of sense. But this chapter of psychology is, on the whole, still open. G. E. Müller, for instance, writes in 1904 that "zu feststehenden Resultaten von allgemeiner Bedeutung haben indessen diese Untersuchungen, die sich in ihren Ergebnissen und Schlussfolgerungen vielfach wiedersprechen, bisher noch nicht geführt."[1] Unless one is prejudiced in favor of some particular theory, one cannot but subscribe to this opinion.

Considerations of this sort led us to begin a systematic investigation of the subject which has extended from the winter of 1903 to the present time. Cutaneous and visual stimuli were used; but, since the former gave uniformly negative results, it has been possible to confine our attention almost exclusively to the latter. We had hoped, likewise, to include auditory stimuli in this series of investigations, but circumstances have rendered it necessary that we make them the subject of future study.

It has become evident in the course of the work that a complete account of the fluctuation of visual stimuli must take into consideration also the fluctuation of the negative after-image. The results of this investigation will be made the subject of a second article, to be followed by a third in which the conclusions of the two preceding articles will be considered from the standpoint of theory and in the light of preceding work. The present study is a reproduction, with some changes, of a paper read before a meeting of experimental psychologists held at Cornell University in March, 1904. It has seemed advisable to publish it in its present form, rather than to wait for a more complete treatment, as was originally planned, because of the evident revival of interest in the problem and the appearance of the recent papers of Dunlap,[2] Killen,[3] and Hammer.[4]

For the sake of clearness, the following order of presentation will be adhered to as closely as possible:

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[1] Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik, 1904, 110.

[2] K. Dunlap: Psychol. Rev., XI, 308.

[3] B. Killen: this *Journal*, XV, 512.

[4] B. Hammer: Zeits. f. Psych., XXXVII, 363; *cf.* C. E. Seashore, *ibid.*, XXXIX, 668.

1st. A statement of theory sufficiently comprehensive to render the results intelligible in terms of it.

2nd. A statement of the lines of investigation.

3d. A statement of results in general.

4th. A statement of results in detail.

I. Visual Stimuli.

(i) *Statement of Theory.* It is our purpose to show in this paper that the intermittences of sensation resulting from minimal visual stimuli which have been referred for explantion to fluctuation of attention are, in reality, simply adaptation phenomena somewhat obscured by the special conditions.

Adaptation is, in itself, a continuous phenomenon, but its continuity is interfered with by eye-movement,[1] blinking, etc. Through these influences, probably essentially through that of eye-movement alone, it becomes an intermittent process, whether the stimulus be liminal or intensive, provided that proper areas be used. The conditions are especially favorable for short periods of intermittence when the stimuli are liminal and of small area.

Eye-movement tends to delay adaptation when the stimulus is liminal and of small area. When the stimulus is much above the limen and the area very small, complete adaptation is prevented, because, under these conditions, no one part of the retina is stimulated long enough to produce the required physiological effect. Also, under such conditions, it is of very short duration, when attained, because a slight shift of the retina is sufficient to produce a complete change in the area stimulated and thus to afford the adapted elements the relief necessary to the revival of sensation. When, on the contrary, the area is very large, these relatively small eye-movements do very little towards relieving the part of the retina stimulated; consequently, complete adaptation takes place much more quickly, and persists apparently indefinitely, unless relief be similarly afforded by some other agency. Areas ranging from 2 mm. to 3-4 cm., viewed at a distance of 1 meter or more, are especially favorable for short periods of intermittence; hence, in the previous investigation of this phenomenon, it is only natural that they should have been chosen and the remainder overlooked.

In all experimental work, however, the conditions that are unfavorable to the production of the phenomenon are as important as, and often more important, for theory, than those more favorable. This proves to be true in the case of what are commonly called 'fluctuations of attention.'

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[1] While working with after-images, this past year, we chanced upon another factor in adaptation, which (so far as we can at present tell) promises to be important. Just how much it bears upon the fluctuation of minimal visual stimuli cannot now be stated. We hope, however, to discuss it fully in the article on the fluctuation of after-images.

Our plan of experimentation, in general, has been to isolate, and test out separately, the probable factors involved, central and peripheral, endeavoring so to vary the conditions as to relieve introspection of any undue burden of analysis. Where the possible factors are numerous and complex, introspective analysis unaided can scarcely be relied upon to solve the problem.

(ii) *Lines of proof.* It is proposed to show:

(1) That involuntary changes in accommodation are not essential factors in the phenomenon. (2) That a stimulus which is not, in itself, intermittent, acting upon the optic centre, does not produce an intermittent sensation. (3) That all liminal stimuli do not fluctuate. (4) That adaptation is an intermittent process under the conditions holding for fluctuation. (5) That adaptation and fluctuation are identical. (6) That adaptation is intermittent chiefly because of eye-movement. (7) That the same correspondence between adaptation and fluctuation obtains in indirect vision.

(iii) *Results: General.* We have the following results to offer at this stage of the work.

A. INVOLUNTARY CHANGES OF ACCOMMODATION ARE NOT ESSENTIAL.

Aphakial subjects experience these fluctuations with apparently no greater variation of phase than can be accounted for on the ground of normal individual differences. Hence, we can conclude that involuntary changes of accommodation play no essential part in the phenomenon.

B. A NON-INTERMITTENT STIMULUS PRODUCES A CONTINUOUS SENSATION.

A minimal and continuous light sensation, produced by electrical stimulation of the cerebro-retinal mechanism, does not fluctuate. Here is a liminal stimulus capable of affecting the optic centre, and pouring in upon it, the effect of which gradually dies out, but shows no signs of intermittence. This fact would seem to indicate that we must look to the periphery for an explanation of fluctuation; for if it were conditioned by central factors, it would be difficult to see why an exception should be made in this case, which is distinctive only in that certain of the peripheral factors which usually modify retinal stimulation are omitted.

C. NOT ALL LIMINAL STIMULI FLUCTUATE.

Liminal visual stimuli of large area, also certain combinations of stimulus and background with very small areas, do not fluctuate.

D. ADAPTATION IS AN INTERMITTENT PROCESS UNDER THE CONDITIONS HOLDING FOR FLUCTUATION.

Adaptation, in general, with areas equal to those with which fluctuations are obtained, is a periodic phenomenon, no matter what the intensity of the stimulus used. The condition of a just perceptible difference between the stimulus and background is favorable, but is by no means essential to the phenomenon. Any stimulus that will completely adapt into its background will do so intermittently within this range of areas; while a stimulus whose qualitative relation to its background is such that it will not disappear completely shows periodic increase and decrease in intensity.

E. ADAPTATION AND FLUCTUATION ARE IDENTICAL.

Whatever conditions relative to the stimulus, or to the combination of stimulus and background, affect the adaptation time, produce a similar effect on the fluctuation time; the effect showing itself either in the phase of visibility, or in the phases of both visibility and invisibility.

Some of the ways by which this correspondence was shown are:

(a) *Fading of the stimulus into its proper gray during the course of a single fluctuation.* In the course of a single fluctuation a colored stimulus is observed to fade into a gray, of a shade depending upon the color used, as always happens in complete color adaptation. Further, the times required for the several colors to fade sustain a very definite relation to their adaptation times. In order of their value from least to greatest, they are (for our stimuli) red, green, blue and yellow. There is little difference, however, in the times required for the disappearance of the residual grays in each case. Moreover, such differences as do occur are chance variations, as is shown by the following averages: red, 1.65 sec.; green, 1.97 sec.; blue, 1.61 sec.; yellow, 1.65 sec. Thus it would seem that the difference in the phases of visibility for these four colors, which is the phenomenon discussed in the next section, does not depend upon their respective brightnesses, but is a duration peculiarity of the processes themselves.[1]

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[1] This point is of two-fold importance. (1) It suggests that the adaptation time of a color is not a function of its brightness; *i. e.*, yellow and red have in no wise different adaptation times because of their positions in the white-black series. (2) It shows that the different visibilities in the fluctuation experiments are not conditioned by the relation of the brightnesses or proper grays of the colors used to the background, but are true expressions of characteristic differences in the color processes themselves.

(b) *Comparison of fluctuation times with adaptation times for colors and grays.* Colors and grays were found to have an order of fluctuation times corresponding to their adaptation times. Four colors, red, green, blue and yellow, gave very different fluctuation periods as compared with each other and with no. 27 Hering gray. The visibility times obtained were in the following order: red, green, blue and yellow, the yellow being nearly four times as long as the red. The complete adaptation times for sheets of the same colors were found to have the same order of length and a rough correspondence as to ratio of length. Further, a striking fact came out with regard to the phases of invisibility. Since red, for example, has a shorter phase of visibility than green, one might naturally expect that its phase of invisibility would also be shorter than the invisibility time of green. The reverse, however, is true. Red has a longer invisibility than green, and this peculiarity is especially marked if one considers the proportionality between the phases, *i. e.*, the ratio invisibility: visibility. The same thing is true of the complementaries blue and yellow. Clearly, we cannot look for a central explanation of this peculiarity; but it seems just what we might expect of adaptation from the standpoint of the compensation theory. The recovery process for the red is the green process. The green process is longer and seemingly more tenacious than the red, as is shown by the adaptation experiments proper, and is further borne out by the longer duration of the green after-image. A similar relation obtains in the blue-yellow process. We have now in progress a series of experiments that will enable us to make an exact comparison of the recovery times for these four colors.

(c) *Combinations of stimulus and background that influence adaptation times correspondingly influence fluctuation times.* By keeping the background constant and varying the stimulus, or conversely, by keeping the stimulus constant and varying the background, a difference in the period of fluctuation was obtained, showing itself chiefly in the phase of visibility. This same thing held in the recognized adaptation experiments. The variations of the phases of visibility and invisibility that were produced in the adaptation experiments were produced also in the fluctuation experiments, the only departure from precise correspondence being that the differences were more marked in the former case, as would be expected from the longer duration of the process.

(d) *Method of areas.* By adequate variation of the area of the stimulus, the phase of visibility was varied from quite long with small areas to nearly zero with large, while the phase of invisibility ranged from very short with small areas, to approximate infinity with large areas, *i. e.*, the faded-out stimulus did not reappear. Thus the phases of visibility and invisibility are, inversely to each other, functions of the stimulus area. The curve representing the phase of visibility starts high on the ordinate and drops down fairly regularly to near the abscissa; while the curve representing the phase of invisibility starts near the abscissa and rises to infinity. The areas chosen do not make the phase of visibility infinite with liminal stimuli; but it is presumable that an area small enough to do this might be found. The curve representing the total period begins high on the ordinate, bends down towards the abscissa, rises again, and passes to infinity. A similar effect, much more marked, was obtained in the adaptation experiments. With the smallest areas used above, the spot never disappeared. Thus the curve representing the whole period starts at infinity, bends down, but not so near to the abscissa as before, rises again, and passes back, but much more irregularly, to infinity. Further, if we took areas sufficiently large, not only did the faded-out stimulus not become visible again under the conditions of fixation observed in such experiments, but it refused to reappear with quite extensive voluntary eye-movements.

Now there seems no way of explaining these results from any peculiarity of function in the centre. In the case of liminal stimuli, the intensities were chosen subjectively equal, consequently there could be no reason for a central discrimination, on the ground of intensity, adequate to account for the wide range of variation obtained; and as for the adaptation experiments, a very flood of vaso-motor waves,[1] etc., would scarcely suffice to wipe out stimuli of so great intensity. If it be argued that it is not fair to attempt to carry over this explanation to the adaptation experiments, we must reply that there would remain, then, the very great difficulty of explaining the close correspondence in the results obtained in the two series of experiments, if entirely different causes were ascribed to the two sets of phenomena. In favor of physiological rhythm, it might be said, however, that there seems a bare possibility of establishing a connection between it and eye-movement. But only in this way could it fit into a theory that should explain all the results cited above. Again, if from any standpoint it be argued that central factors[2] are involved in eye-movement, blinking, etc., and that these influences, therefore, make for a central theory, we reply that the movements are more likely to be reflex, made in sympathy with the changes and needs of the retina. But granted that they are central, they still play no greater part in the explanation of the phenomenon than is the case with adaptation in general. The part played by all of these factors becomes a common problem, to be investigated in connection with adaptation, and not substituted for it as a *vera causa*. There seems, likewise, little chance of explanation of these results from the side of attention. In fact, they seem to be precisely contradictory of any theory that seeks its account from this source. Increase in area of the stimulus is presumed to be equivalent to an increase in intensity, as regards its noticeability or its efficiency[1] for attention. Efficiency, or whatever may be considered as its equivalent in these results, let the criterion be what it will, is reduced to a minimum.

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[1] J. W. Slaughter: this *Journal*, XII, 313.

[2] E. A. Pace: Philos. Studien, VIII, 388; XX, 232.

F. ADAPTATION IS RENDERED INTERMITTENT CHIEFLY BY EYE-MOVEMENT.

That eye-movement[2] is chiefly responsible for the intermittence of adaptation seems evident from the results. Blinking might enter in, as an occasional factor, to delay adaptation or cause the reappearance of the faded-out stimulus; but it is much too infrequent to explain all of the reappearances. Besides, it could offer no explanation for the difference in the times of visibility and invisibility for the different areas, since the amount of relief afforded in each case would be the same. Eye-movement alone seems adequate to do this.

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[1] There are, probably, two factors which give an increased area an increased efficiency for attention. (1) There is an actual increase in the intensity of the sensation. For example, when our stimulus was obtained by light transmitted through opal glass, the source of light had to be moved farther away in order to give a liminal effect when the area of the stimulus diaphragm was increased. Likewise, when the stimulus was seen by reflected light, the opal glass plate had to be moved farther out from the background when the area of the stimulus was increased. (2) The increased area occupies more of the field of vision; hence the rival area is not only of less extent (fewer distracting factors, etc.), but is pushed more and more into the field of indirect vision. The first factor was ruled out by decreasing the intensity of the stimulus until it was liminal. The second, however, operated to give our large areas greater efficiency for attention. But in spite of this, the large areas, although favoring rapid adaptation, gave us minimal visibility. Besides helping to make our point for adaptation, this result serves as a striking illustration of how little the central factors avail against the peripheral in so-called sensory attention.

[2] The other factor conditioning adaptation is probably, likewise, essentially dependent upon eye-movement. At least, the modification which gives it a bearing upon this problem is caused by eye-movement. The effect produced is, similarly, a freshening of the adapted elements, and will be understood, throughout the discussion, to supplement the change produced by shifting the adapted elements into a region of different stimulation. Since its action in point of time follows immediately upon eye-movement, and does not change, but only supplements, the restoration produced by change of stimulation, there was little apparent need of it to explain the results of the following tables. Hence, had it not come to light in the work on after-images, it probably would have been entirely overlooked.

That eye-movement produces its effect in the manner we have stated probably needs further proof. Hess[1] has contended that a spot once adapted-out will not reappear so long as fixation is held perfectly steady; but his experiments do not indicate how eye-movement causes reappearance. MacDougall[2] explains the effect of eye-movement upon the reappearance of minimal visual stimuli on the basis of innervation. Innervation, however, could not account for phases of invisibility ranging from nearly zero to infinity; besides which, extensive voluntary eye-movements were wholly ineffective to revive sensation in the case of the largest areas used. Similarly, the mechanical effects of pressure, etc., are ruled out. Hence we seem not only warranted, but forced, to fall back for explanation upon an actual shift of the adapted elements away from the area of stimulation.

A more direct experimental confirmation, than was afforded by the method of variation of areas, of the view that eye-movement interferes with the course of adaptation, and is also the conditioning factor for the wide range of variability found in the phases of visibility and invisibility in the fluctuation experiments, is given by the following results. An examination of the average frequency of eye-movement in the horizontal and vertical planes during fixation showed that three of our observers had a marked excess in both frequency and range in the horizontal, while the fourth had an excess of frequency in the vertical, but of range in the horizontal plane. This appeared to mean that, for three observers, there was a greater change of stimulation, and consequently greater relief for the adapted elements, in the horizontal than in the vertical direction, while the reverse was true, though probably to a less degree, for the fourth. To test this interpretation, stimuli longer than broad were used,[3] *e. g.*, slips of paper 5 mm. x 40 mm. When these were placed with the longer dimension vertical, the shorter dimension would fall in the direction of greater unsteadiness of fixation for the three observers who had the excess of eye-movement in the horizontal plane. Consequently, a maximal interference with adaptation for these stimuli would be obtained, and one might expect an increase in the phase of visibility and a decrease in the phase of invisibility. On the other hand, if the longer dimension were placed in the horizontal and the shorter in the vertical plane, a minimal interference possible to these stimuli would be secured, and a decrease in the phase of visibility and an increase in that of invisibility should ensue. For the fourth observer, with the stimulus arranged as described above, the reverse should be true; but probably not in so marked a degree, since his range was greater in the horizontal, and this fact to a certain extent counteracted the effect of frequency. This observer also had an astigmatism in the vertical plane, which caused the stimulus to become spreading and diffuse in the horizontal, a result equivalent to greater breadth for adaptation.

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[1] C. Hess: *von Graefe's Archiv*, XL, 2, 274.

[2] W. MacDougall: *Mind*, XI, 316; XII, 289.

[3] This procedure was suggested by Professor L. Witmer, of the University of Pennsylvania.

That these methods of arrangement of stimulus caused a marked change in the phases of visibility and invisibility for each observer will be seen by inspection of the Tables. Indeed, the correspondence between the quantities: $\frac{\text{visibility} \div \text{invisibility}}{\text{visibility}^1 \div \text{invisibility}^1}$ and $\frac{\text{frequency}}{\text{frequency}^1}$ is much closer than was anticipated.

G. CORRESPONDENCE OF ADAPTATION WITH FLUCTUATION IN INDIRECT VISION.

To show that fluctuation in indirect vision is not a special phenomenon, but that the correspondence between adaptation and fluctuation obtains here as well as in direct vision, the following set of experiments was carried out. (1). Beginning with direct vision, a liminal stimulus was moved successively 4, 8, 12, 16, etc., cm. towards the periphery, and records were obtained at each point. A parallel set of records was obtained with the same stimulus at full intensity. Both sets of records showed a fairly regular decrease of visibility and increase of invisibility as the stimulus was moved towards the periphery. The adaptation times obtained in a separate series of experiments with the same stimulus also showed a corresponding decrease from direct vision to periphery.

(2). An increase of area with liminal stimuli in indirect vision gave a decrease of visibility and an increase of invisibility, very much the same as was obtained for direct vision.

There seems little doubt that all the results secured for direct vision could have been paralleled for indirect vision. The above series, however, satisfied us that the phenomenon here is essentially the same. It seems, then, that the conclusion is justified that adaptation causes the disappearance of the stimulus, and unsteadiness of fixation the wide range of visibility and invisibility in case of different areas, and the restoration when complete adaptation has set in; and that this effect is due to relief of adapted elements by actual shift away from the area of stimulation, or rather into a region of different stimulation.[1]

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[1] Together with the supplementary factor mentioned but not specified above,—if this prove to have the efficacy which we now incline to ascribe to it.

H. FACTS OF MINOR IMPORTANCE.

The following facts of minor importance may also be cited.

(a) *Result of increasing the distance of the observer.* The effect of increasing the distance of the observer from the stimulus was tried The area subtended by the stimulus on the retina follows the law of inverse squares. Although the phase of visibility increased and the phase of invisibility decreased with the increase of the observer's distance, still the results did not at all closely follow those obtained by the corresponding variations of area observed at a distance of 1 meter. The phase of invisibility increased much more rapidly with the increase of distance than was demanded by the law of inverse squares. This seems to argue in favor of eye-movement; for the greater the observing distance, the greater is the shift of the adapted elements away from the stimulating area with each eye-movement; hence the greater is the interference with the course of adaptation.

(b) *Connection between reappearance, and conscious eye-movement and blinking.* Experiments for recording[1] the connection between reappearance and conscious eye-movement and blinking showed coincidence in from one-third to one-half the total number of cases.

(c) *Effect of moving the eyes voluntarily.* Records of series in which an observer purposely moved his eyes at short intervals showed very few fluctuations. Another observer was directed to relieve the strain when, and as, impulse directed. No fluctuations were experienced in one revolution of the drum: 102 secs.

(d) *Effect of momentary cessation of the stimulus.* Anything else that temporarily relieved the retina, such as the interposition of some object between the source of light and the screen when the spot was made visible by transmitted light, caused reappearance when the spot had vanished, and delayed disappearance when the spot was visible.

(e) *Influence of practice.* An inexperienced observer usually obtained longer times of visibility and shorter times of invisibility until a certain stage of practice was reached. Some, indeed, were unable at first to get fluctuations at all. This is precisely what would be expected as the result of unpractised fixation upon adaptation. Further, the result seems incompatible with the theory of fluctuation of attention, for one would expect practice to increase, certainly not to diminish, efficiency of attention.

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[1] The method of recording was simple. When reappearance came with conscious eye-movement or blinking, *O* substituted for the usual release of the key an extra pressure and immediate release. With practice, this method offered little if any distraction.

Again: towards the close of a sitting, with one of our observers, the phase of visibility began to lengthen and the phase of invisibility to decrease very perceptibly: in a few cases so much so, that disappearance did not come at all. At these times the observer complained of eye-fatigue and inability to fixate steadily. This result, too, testifies for adaptation and against central factors.

(f) *Introspective evidence.* Introspection also furnishes valuable evidence. For all observers the spot faded gradually,[1] and as this process went on the strain of attention increased, reaching its maximum with the disappearance of the stimulus, and continuing until reappearance, when momentary relief was experienced. The natural attitude of our observers seems to have been to hold the sensation as long as possible. Hence it was to be expected that the strain should increase with the decrease of the sensation. Had their attitude been different, had they, for instance, been instructed that disappearance was the thing to be expected and attained, it is possible that relief might have come with invisibility: that relaxation of attention might then have ensued. Even so, it would have been the result and not the cause of the disappearance. Moreover, the conditions of the experiment make for stimulation rather than for fatigue of attention. The constantly changing stimulus, the unexpected reappearances, etc., are attention-compelling to a high degree. There is no monotony. There are rather elements of fascination. As one observer stated, "one is always on the alert to see what will happen next." In fact, were we endeavoring to demonstrate an unwavering attention, scarcely a better set of conditions could have been selected.

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[1] The conflicting reports on this point in the literature have probably been due to the peculiar difficulties attending observation with the Masson disk. With stationary stimulus and background, such as were used by us, there is no doubt that the stimulus disappears gradually. If, on occasion, the actual change in intensity could not be detected, the disappearance was gradual and progressive in point of area, the background encroaching upon the stimulus from one direction or another. More will be said about this type of disappearance in a later article.

If it be contended that stimuli whose intensity can be detected in decrease are not liminal, we reply that, in practice, just noticeability is not so consistently obtained that the detection is impossible. We have worked most carefully to get this degree of intensity, approaching the point from either direction, and still the observer would report, during the course of the fluctuation, that the stimulus faded out. What holds of our stimuli has probably held also of others; for we undoubtedly succeeded in getting finer adjustments of intensity with the arrangement finally adopted than was possible with the Masson disk. With the Masson disk itself fading was recorded by our observers.

Dealing, as we did, with many degrees of intensity, facility for judging intensity changes was naturally acquired. During this time, besides, two of our observers were regularly working on the determination of visual limens.

It will be understood here, as elsewhere in the discussion, that our contention is not that attention does not fluctuate.[1] That is a question aside. We are merely concerned with showing that certain phenomena, that have usually been attributed to fluctuation of attention and cited as its classical demonstration, are to be otherwise explained. That there is fluctuation on the content side of consciousness goes without saying: the sensation comes and goes. But we believe not only that this fluctuation is to be explained wholly by reference to the sense process, but also that the associative factors that aid in the exaltation of the sensation are all the more active because of this sinking of the content below the limen on the peripheral side. We may add then that, in so far as the facilitation of the process elevated to prominence, or the inhibition of other processes, depends upon associative factors, it should be maintained that the conditions of these experiments make for an exalted and sustained attention.

(iiii). *General description of method and apparatus.* Before going more into detail as to method and results, we may remark that all devices that did not produce decided changes of result have been considered as worthless for yielding evidence in a case where without change in the experimental conditions the variations are so considerable. One finds cited in the literature, as due to some change in method or in support of some particular theory, variations no greater than our records showed from day to day without any change in the experimental conditions. There is, in dealing with this problem, especial need for clearly cut and decisive methods of experimentation, as well as for extreme caution in referring slight changes in result to a variation of experimental conditions.

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[1] In so far as attention is considered as a state or mode of consciousness, it may be said to fluctuate. But interpreted in this sense, it is ruled out for purposes of explanation. We must look, instead, to the processes concerned in giving this particular state or mode to consciousness. The above paragraph is written from the point of view of the central processes involved. If we are to investigate the action of these processes, it would be well to have consciousness as purely central as possible, *i. e.*, ideas should be worked with, instead of sense perceptions. The changing content given by the peripheral process is fatal to the determination whether the central processes will act continuously for any length of time in a given relation.

In general, we probably recognize too little the difference between attention where the content is peripheral, and attention where it is central. The distinction should undoubtedly be made in any discussion of fluctuation.

We have even found it necessary not to be obliged to compare results obtained at different sittings, because of the subjective changes that occurred from time to time in spite of experimental control. Our comparisons have, therefore, been planned in series to be finished at a single sitting, and the order of their presentation has been changed so as to compensate as much as possible for probable changes in the condition of the eyes and fixation-apparatus from the beginning to the close of the period. Series, then, were compared from day to day, rather than the members needed to make a single series.

For registration, throughout all of the work, a Ludwig-Baltzar kymograph was used; together with a Marey tambour and bulb, whereby the entire course of the fluctuation as well as mere appearance and disappearance could be traced, when desired; and an electromagnetic time-marker in circuit with a metronome, enclosed in a soundless box. All of this apparatus was screened from the observer by a sliding curtain. The work was done mostly in a long room, the 'reaction room,' with the windows all at one end. Thus cross-lights, unequal illumination of the background, etc., could be avoided. The observer sat with his back to a high window and his head in a head-rest fastened to the edge of a long table, along which the frame bearing the stimulation apparatus was moved as required. The time unit throughout is 1 sec.

(v) *Results.* (*In detail.*) It is scarcely necessary to mention that the results, unless otherwise stated in the tables, are averages obtained from a large number of records. In the main, throughout the work, they were confirmed not only by the writer and the observers cited: *viz.*, Misses Fitch (*F*) and George (*Ge*) and Messrs. Sabine (*S*) and Galloway (*Ga*), but also by a number of the students of the junior training course, either as a part of their regular work, or as substituted for it. Where results have not been obtained from all of the regular observers, this has been due solely to lack of time. *S* and *Ge* gave the least and *Ga* the longest time to the work. All four observers were students in the department of psychology, and had had laboratory training. *Ga* had also had experience with the problem, both as experimenter and observer, at the University of Michigan.

A. INVOLUNTARY CHANGES OF ACCOMMODATION ARE NOT ESSENTIAL.

Two aphakial subjects were experimented upon. One of them had so little accommodation that words in fine print could not be moved more than 2 mm. farther from or nearer to his point of clearest vision (determined by the focus of his glasses) without becoming less distinct. His head was clamped in a head-rest, and the card slid along a meter rod at the level of his eyes in the median plane. Every precaution was taken to secure accuracy. It may safely be said that the man was practically without accommodation. As was stated before, the results obtained from both of these men were uniformly negative, *i. e.*, no greater variations were found than can be explained on the ground of normal individual differences.

B. A NON-INTERMITTENT STIMULUS PRODUCES A CONTINUOUS SENSATION.

The well-known fact that make or break of a direct current produces a flash of light, if the electrodes are properly applied, led us to believe that, if the current were rapidly interrupted, these flashes might be caused to fuse into a continuous sensation. This proved to be true. An interrupter so constructed that six makes and breaks occurred with every revolution of the interrupting cylinder was used. It was driven by a motor, and its speed of revolution was regulated by a transformer,[1] so finely graduated that a change of a single interruption could be obtained. In circuit with the observer and the battery was inserted a resistance rack of German silver wire, also a Westinghouse ammeter graduated in milliamperes. By this arrangement it was possible to keep the current flowing through the circuit absolutely constant. A speed indicator was also used. This was rendered necessary for the double reason that the quality of the stimulus depended upon the rate of interruption, and that any change in the rate influenced the amount of current flowing through the circuit. This latter phenomenon was probably due to induction effects in the coils of wire used. The number of Leclanché cells required to produce the stimulation was usually eight, although as few as four and as many as twelve were used for different observers. The current flowing through the circuit, when liminal effects were obtained, ranged from one to two milliamperes. The one electrode was placed in the hand and the other, a sponge electrode, above the eye on the nasal side. The observer was stationed in the dark-room, and allowed to adapt. Then the current was applied, and carefully worked down until liminal effects were produced. The sensation chosen for observation was of the nature of an irregular patch or cloud of light, varying in color for different observers through violet, blue, and yellow.

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[1] The speed-transformer, made to our order, was in the form of a segment of a cone, with a grooved surface for the retention of the motor and interrupter belts. The dimensions of the segment were such that the decrease in circumference from groove to groove was very small. This arrangement, together with graduated pulleys on the motor and interrupter, made very slight changes of speed possible.

The interrupter consisted of two brass cylinders with six equal open and closed spaces on either surface. An insulating cross-section separated the two cylinders. This duplicate arrangement was not necessary, except that it made the connections more convenient for our purpose, and that, by a proper setting of the brushes, the instrument could also be used as an alternator. The motor and interrupter were mounted on sliding frames, in order that the belts might be kept taut.

The sensation, when liminal, usually lasted about 30 seconds, gradually fading out, and in no case reappearing however long the current was applied. The effects obtained at different rates of interruption show differences. Lower rates usually produced a series of flashes, in which more or less irregular patterns were made out. A little higher rate produced bars on a colored background. With a still higher rate, the bars assumed a radial position around a dark opening fringed with colored light. Here began the transition stage. An increase now gradually changed the effect to an uniformly colored field. This fusion usually came at rates ranging from 85-100 interruptions per second. One observer at 80 saw a dark violet field; at 85, purple; at 100, blue; and from 120-162, yellow. It would be interesting to discover whether there is a definite order in the succession of colors for all observers as the rate is increased. The point being merely incidental to our purpose, the investigation was not carried far enough to determine this.

That the retina is stimulated is indicated by the following experiment. A rate of interruption was chosen that would produce bars. The observer stimulated each eye separately and noted the patterns obtained. Then the electrodes were applied above both eyes simultaneously. It so happened that the bars for one eye were inclined towards the horizontal, and for the other towards the vertical. When both eyes were stimulated at once, and the fields superposed, the two patterns still remained distinct, with the bars set obliquely to each other. As to whether the visual substance was involved, the experiment showed that there was always an after-effect, which behaved much as after-images do. However, there was rarely any trace of complementary coloring. In any event, the result goes to prove that a continuous stimulation, reaching the optic centre, does not produce an intermittent sensation.

The data of C. and D. are, for convenience, subsumed under E., to aid in showing the correspondence between adaptation and fluctuation.

C, D, E. ADAPTATION AND FLUCTUATION ARE IDENTICAL. CORRESPONDENCE IS SHOWN BY:

(a) *Fading of the stimulus into its proper gray during the course of a single fluctuation.* A Masson disk of the standard dimensions was used. The colors (Hering standard) were red, green, blue, and yellow. The background was neutral engine-gray, darkened by 180° of velvet black.

The change into a gray differing from the background was first reported by Dr. Bentley. Better to bring out the phenomenon, a comparison ring of gray was made concentric with the colored ring. The judgment was difficult, and it has not been possible as yet to repeat the experiment under more favorable conditions. The grays into which the colors changed were judged of different brightnesses in the order, from least to greatest, of blue, red, green, and yellow. These grays correspond to those obtained when these particular colors, saturated, were adapted down.

Adaptation, then, evidently carries the colors to the limen. That it is also adequate to get rid of the gray remaining cannot be questioned. Consequently, it does not seem necessary to supply another process to complete the disappearance, especially when there is nothing in the course of the phenomenon to indicate the need of such a supplement. Introspection shows the change from start to finish to be uniform and continuous. It will be noticed that the difference between the total phase of visibility for the four colors in Table I is not nearly so great as it is in Tables II, III, IV, and V. The recovery-peculiarities characteristic of adaptation are also much less noticeable in the phases of invisibility. This difference in result is always found when the data for the Masson disk and the stationary system are compared.

TABLE I.[1]

Ga. Fading of a color into its proper gray during the course of a single fluctuation.

Stimulus	Number of Fluctuations	Changes to Gray	Vis. Color	Vis. Gray	Invis.	Order of Brightness of Gray
Red, 2x5 mm.	21	8	2.4	1.65	1.99	3d.
Green, " "	14	7	2.99	1.97	1.79	2d.
Blue, " "	13	7	3.57	1.61	2.04	4th.
Yell'w," "	14	6	3.70	1.65	2.02	1st.

(b) *Comparison of adaptation time and fluctuation time for colors and grays.* (1) *Fluctuation.* Squares of paper were pasted upon gray card-board and placed behind an opal glass plate. They were thus seen by reflected light through the opal glass. The intensity was easily regulated by slight changes in the distance of the plate from the card-board. Different makes of standard colors were used at different times. The stimuli for the following Tables were cut from Milton-Bradley papers. The size of the squares was, in each case, 2 cm. x 2 cm., and the distance of the observer 1 meter. All other conditions were the same throughout.

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[1] The writer must apologize to the reader for the ragged appearance of this and the following Tables, owing to the various number of decimal places to which the calculations have been carried out; and must also deprecate any claim to especial accuracy in the case of the longer decimals. He had intended to round-off the figures to two places, but this was inadvertently omitted. Rather than delay the printers, he has allowed the Tables to stand as they were in MS.

TABLE II.

Ga. Comparison of fluctuation time with adaptation time using colors and grays. Fluctuation: showing that visibility and invisibility have characteristic adaptation and recovery peculiarities.

Stimulus	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Invis.: Vis.	Period
Gray, 2x2 cm.	4.34	1.045	3.50	.727	1.24		7.84
Red, " "	2.26	.53	3.48	.108	.649	1.539	5.74
Green, " "	3.71	.954	3.058	.80	1.213	.824	6.768
Blue, " "	5.68	1.244	3.755	1.181	1.513	.661	9.435
Yellow, " "	8.375	2.075	3.46	.337	2.42	.413	11.835

TABLE III.

S. Comparison of fluctuation time with adaptation time using colors and grays. Fluctuation: showing that visibility and invisibility have characteristic adaptation and recovery peculiarities.

Stimulus	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Invis.: Vis.	Period
Gray, 2x2 cm.	3.47	1.053	2.31	.533	1.502		5.78
Red, " "	1.333	.338	2.8	.527	.4761	2.025	4.133
Green, " "	2.806	.506	2.431	.575	1.154	.866	5.237
Blue, " "	3.2	.836	1.779	.326	1.799	.562	4.979
Yellow, " "	5.576	1.257	1.238	.537	4.504	.221	6.814

TABLE IV.

Ge. Comparison of fluctuation time with adaptation time using colors and grays. Fluctuation: showing that visibility and invisibility have characteristic adaptation and recovery peculiarities.

Stimulus	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Invis.: Vis.	Period
Gray, 2x2 cm.	3.715	.693	2.028	.917	1.832		5.743
Red, " "	1.566	.383	5.95	1.608	.2632	3.799	7.516
Green, " "	3.17	.96	5.6	2.41	.566	1.767	8.77
Blue, " "	4.471	1.157	5.042	1.142	.886	1.127	9.513
Yellow, " "	7.2	1.563	1.354	.355	.288	.188	8.554

TABLE V.

F. Comparison of fluctuation time with adaptation time using colors and grays. Fluctuation: showing that visibility and invisibility have characteristic adaptation and recovery peculiarities.

Stimulus	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Invis.; Vis.	Period
Gray, 2x2 cm.	3.8066	.786	2.54	.466	1.498		6.3466
Red, " "	2.4812	.287	3.362	.568	.737	1.355	5.843
Green, " "	3.55	.462	2.85	.443	1.245	.802	6.40
Blue, " "	3.586	.653	2.793	.7	1.283	.779	6.379
Yellow, " "	4.716	.791	2.141	.366	2.202	.453	6.857

Attention is called again to the fact that, as would be expected from the compensation theory, red and blue have longer phases of invisibility and shorter phases of visibility, respectively, than green and yellow. The relative value of the invisibilities as compared with the visibilities in each case is expressed by the ratio invisibility : visibility.

(2) *Adaptation.* To test the correspondence of these results with those obtained from adaptation, sheets of colors of the same make were placed behind lightly frosted glass and observed at distances ranging from 2-3 meters. Just how much the intensity was lowered by these conditions we are not able to say,—probably not one half. This does not matter, however, so long as each color was tested under precisely the same conditions, since only comparative values were wanted. The following results were obtained:

Obs. *G.* Distance: 235 cm. Time unit: 1 sec.

Red	41
Green	55
Blue	78
Yellow	263

Because of the severe eye-strains, the intensity was further reduced for *F* by placing the color 11 cm. behind the frosted glass.

Obs. *F.* Distance: 235 cm. Time unit: 1 sec.

Red	25
Green	41
Blue	58
Yellow	225

For *S*, the color was placed 19 cm. behind the frosted glass.

Obs. *S.* Distance: 300 cm. Time unit: 1 sec.

Red	19
Green	52
Blue	160
Yellow	196

The order is the same as was obtained in the fluctuation experiments; and a comparison of the Tables will show that a rough correspondence holds in the ratios sustained between the phases of visibility and the adaptation times in each case.

It may be objected that the colors used were not standardized. We are, however, not attempting to state results for standard colors. Our sole aim is to show correspondence between adaptation and fluctuation. This has been accomplished by using identical colors in the two sets of experiments. It could have been done no better, we believe, by using standard colors.

(c) *Combinations of stimulus and background that influence adaptation time correspondingly influence fluctuation time.* (1) *Fluctuation.* For this point so far the Masson disk has been used. From all the colors tried as background, light greenish blue (Hering), yellowish green (Milton-Bradley), yellow (Milton-Bradley), orange (Hering), gray, and in one case dark red (Milton-Bradley) were selected for the following Tables. The stimulus strips were 2 mm. x 5 mm., and were placed 8 mm. apart along the radius. They were, with one exception, of Hering red.

This method we consider very unsatisfactory. In the first place, results never stand out so clearly with the Masson disk as when the system is at rest: judgments are difficult; distractions are many, and gradations of intensity, neither so constant nor even so delicate, can be obtained. And, secondly, as our disks were made, the stimulus color was rendered liminal by mixing with the color of the background rather than with a gray of its own brightness. If we take, for example, a red stimulus upon a light blue background, the effect obtained was a faintly reddish blue upon a blue background, slightly differing from it in brightness. But even this approximation to the desired conditions was sufficient to vary the phase of visibility to a rough correspondence with the results obtained with a similar combination of stimulus and background in the adaptation experiments.

(2) *Adaptation.* Here, likewise, red at *full intensity* disappears most readily upon the light blue; not quite so readily upon the gray used; and never entirely goes into the background, although the color is lost periodically, upon the orange, yellow, and yellowish green. Yellow on dark red is peculiarly persistent.

In addition to the combinations here used, we have tried a number both of grays and of colors, and are satisfied that whatever alters the conditions for adaptation correspondingly alters the conditions for fluctuation.

(d) *Method of variation of areas.* (1) *Fluctuation.* This

TABLE VI.

Ga. Combinations of stimulus and background that influence adaptation time correspondingly influence fluctuation time. Fluctuation.

Stimulus	Background	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Red, 2x5 mm.	Light Blue	2.287	.681	3.362	.631	.6803	5.649
" " "	Yellow	4.609	.1141	2.78	.600	1.6856	7.388
" " "	Orange	4.709	.1127	2.936	.663	1.600	7.645
" " "	Yellowish Green	4.130	1.161	2.715	.800	1.521	6.845
" " "	180° Engine Gray + 180° Velvet Black	3.615	1.076	3.268	1.1	1.106	6.893
Yellow, 2x5 mm.	Dark red	6.512	1.012	3.887	.912	1.675	10.399

TABLE VII.

F. Combinations of stimulus and background that influence adaptation time correspondingly influence fluctuation time. Fluctuation.

Stimulus	Background	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Red, 2x5 mm.	Light blue	2.9	.398	3.164	.58	.969	6.064
" " "	Yellow	4.73	.52	2.96	.4	1.598	7.69
" " "	Yellowish Green	4.4	.717	3.2	.37	1.375	7.60
" " "	180° Engine Gray + 180° Velvet Black	2.9	.292	2.95	.628	.983	5.85

method was tried both upon the Masson disk and with the opal glass plate as a background. The results in both cases were unquestionable; but, as before, those given by the stationary system were much the more satisfactory and much the more clearly cut. Because of this, and chiefly because the disk did not permit enough variation of area, the Masson disk will be omitted from further consideration in this paper.

A stimulus was obtained upon the opal glass plate by light coming from a bank of lamps behind it, passing first through a plate of frosted glass, then through the opal glass itself. The magnitude of the stimulus was regulated by a card-board diaphragm behind the screen; its intensity, by varying the distance of the lamps, also by means of a curtained window in front. This photometric arrangement provided a very sensitive means of obtaining a just noticeable stimulus. After the initial adjustment was made, great care was taken that the illumination of the background should remain constant throughout the experiment.

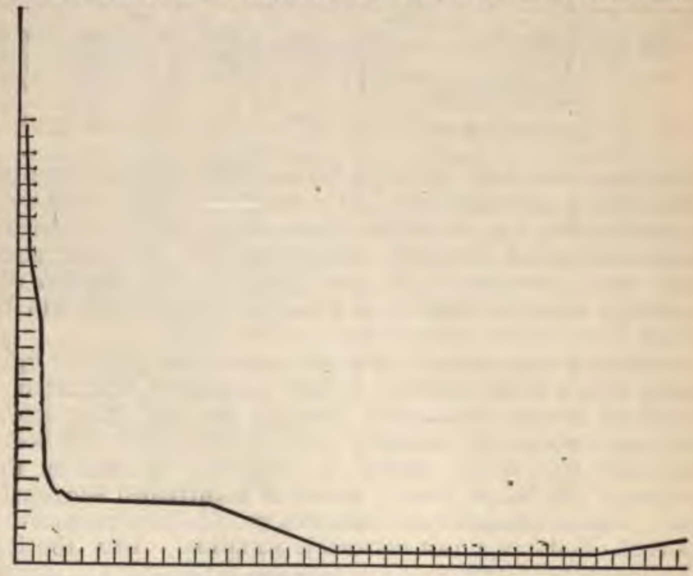
TABLE VIII.

F. Method of variation of areas. Fluctuation: showing inverse variation of visibility and invisibility with increase of area.

Area	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
2x 2 mm.	16.86	4.83	.8	.28	21.075	17.66
4x 4 "	12.93	3.53	1.15	.38	11.24	14.08
6x 6 "	10.78	3.8	2.9	.83	3.71	13.68
8x 8 "	5.65	1.53	2.76	.52	2.23	8.41
10x10 "	4.74	1.32	2.34	.63	2.023	7.09
12x12 "	4.22	.97	2.72	.57	1.551	6.94
14x14 "	4.35	1.29	3.2	.46	1.359	7.55
16x16 "	3.96	1.22	2.918	.59	1.360	6.878
6x 6 cm.	3.73	1.11	5.2	.77	.717	8.93
10x10 "	.81	.23	9.65	2.8	.073	10.46
14x14 "	.8	.5	29.46	4.66	.027	30.26
18x18 "	.85	.45	40.25	2.25	.021	40.10
22x22 "	1.4	No reappearance.				

For *F*, beginning at areas ranging from 10 cm. x 10 cm.—14 cm. x 14 cm. in the different records, it was noticed that only the edge of the lower left hand corner and left side reappeared.

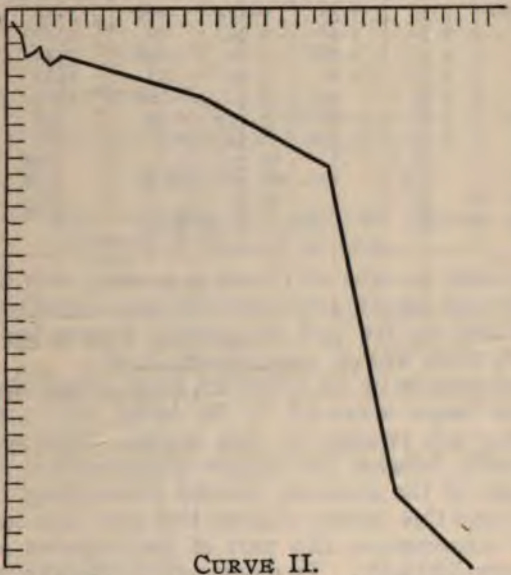
The results of this Table have been thrown into the form of



CURVE I.

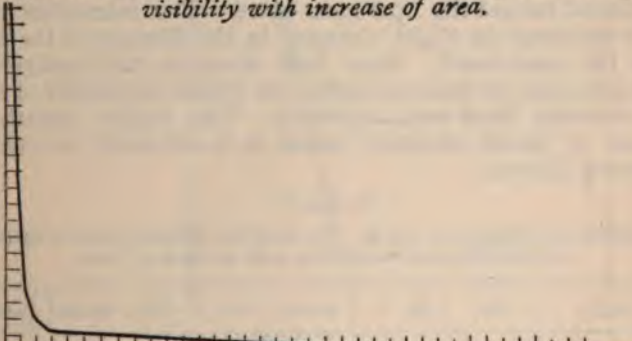
Curve for visibility. TABLE VIII. *Showing decrease of visibility with increase of area.*

a curve. The dimensions of the stimulus are laid off along the abscissa, millimeter for millimeter; the time values along the ordinate, on the scale of 1 second to 5 millimeters. The last and more horizontal part of the curve for visibility represents the reappearance of the edge of the left side and the lower left hand corner.



CURVE II.

Curve for invisibility.[1] TABLE VIII. *Showing increase of invisibility with increase of area.*



CURVE III.

Curve for visibility : invisibility. TABLE VIII. *Showing decrease with increase of area.*

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[1] In this and the following curves invisibility is platted as a negative quantity.

TABLE IX.

Ga. Method of variation of areas. Fluctuation: showing inverse variation of visibility and invisibility with increase of area.

Area	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
2x 2 mm.	7.3	1.31	2.122	.466	3.44	9.422
4x 4 "	6.34	1.38	2.94	.46	2.16	9.28
8x 8 "	4.53	1.066	3.24	.558	1.39	7.77
12x12 "	4.23	1.45	3.75	.53	1.12	7.98
16x16 "	4.03	.933	3.9	.708	1.03	7.93
6x 6 cm.	1.83	.58	5.675	1.89	.32	7.505
10x10 "	1.19	.436	6.96	2.8	.17	8.15
16x16 "	.61	.1	12.53	3.3	.048	13.14
26x26 "	.725	.175	25.36	6.067	.028	26.085
34x34 "	.55	.2	25.13	4.3	.021	25.68
42x38 "	.8	No reappearance.				

For *Ga*, with an area of 10 cm. x 10 cm., only about one third of the area covering the lower left hand corner reappeared. From that area on, the part reappearing became less and less, until finally there was no reappearance at all.

The reappearance of the lower left hand corner alone in the case of the larger areas led to the belief, after a time, that the stimulus was stronger in this region. This was all the more probable, because the window was somewhat above and to the right of the observer, thereby illuminating the background around this corner slightly less than the rest of the field. In consequence, this part of the stimulus stood out slightly supraliminally. To obviate this difficulty, stimuli of Hering gray, no. 27, were pasted upon engine-gray card-board and placed behind the opal glass plate. The intensities were easily regulated by slight changes in the distance of the plate from the card-board. Since both stimulus and background were now seen by reflected light, the former inequality of relation between them was impossible. The rather remarkable change of results obtained makes it worth while to note the following Tables.

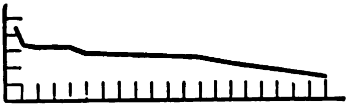
TABLE X.

F. Method of variation of areas. Fluctuation: showing inverse variation of visibility and invisibility with increase of area.

Area	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
3x 3 mm.	4.435	.742	4.285	.835	1.035	8.72
6x 6 "	3.372	.854	6.772	1.218	.498	10.144
10x10 "	3.327	.5	11.562	1.937	.287	14.889
20x20 "	3.31	.71	12.975	1.84	.255	16.285
25x25 "	2.962	.427	14.36	2.812	.206	17.322
6x 6 cm.	1.533	.333	38.00	.723	.0403	39.533
10x10 "	1.145	No reappearance.				

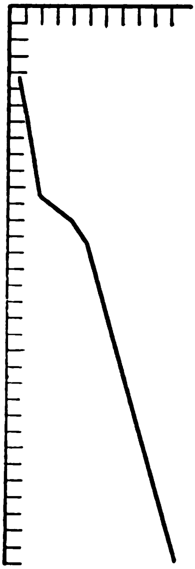
Curves were plotted from these results to compare with those obtained from Table VIII.

It will be noticed that this series began with an area of 3 mm. x 3 mm. In the former case, it was 2 mm. x 2 mm.



CURVE IV.

Curve for visibility. TABLE X. *Showing decrease of visibility with increase of area.*



CURVE V.

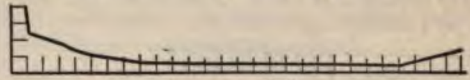
Curve for invisibility. TABLE X. *Showing increase of invisibility with increase of area.*

TABLE XI.

Ga. Method of variation of areas. Fluctuation: showing inverse variation of visibility and invisibility with increase of area.

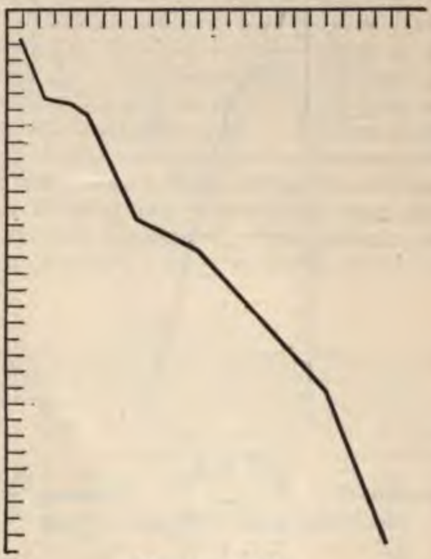
Area	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
4x 4 mm.	4.466	1.26	1.853	.406	2.41	6.319
6x 6 "	2.385	.910	2.934	.828	.812	5.319
12x12 "	2.000	.537	5.257	1.600	.380	7.257
16x16 "	1.984	.761	5.269	1.768	.376	7.253
20x20 "	1.453	.615	5.557	2.050	.261	7.010
25x25 "	1.122	.321	6.100	1.538	.183	7.222
4x 4 cm.	.775	.225	12.583	3.266	.061	13.358
6x 6 "	.716	.200	14.460	4.216	.049	15.176
10x10 "	.632	.197	23.100	3.500	.027	23.732
12x12 "	.590	.284	33.866	7.445	.017	34.456
14x14 "	1.5	No reappearance.				

The following curves represent the results of the preceding Table. The first area used is 4 mm. x 4 mm.



CURVE VI.

Curve for visibility. TABLE XI. *Showing decrease of invisibility with increase of area.*



CURVE VII.

Curve for invisibility. TABLE XI. *Showing increase of invisibility with increase of area.*

TABLE XII.

Ge. Method of variation of areas. Fluctuation: showing inverse variation of visibility and invisibility with variation of area.

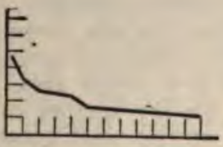
Area	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
3x 3 mm.	5.43	1.5	3.192	1.169	1.732	8.622
6x 6 "	3.14	1.033	4.3	.930	.730	7.44
10x10 "	2.469	.953	6.09	.305	.305	8.559
20x20 "	2.16	.72	15.683	4.016	.137	17.843
25x25 "	1.185	.255	18.342	3.342	.064	15.75
6x 6 cm.	.7	.133	27.75	5.85	.025	28.45
10x10 "	1.2	No reappearance.				

TABLE XIII.

S. Method of variation of areas. Fluctuation: showing inverse variation of visibility and invisibility with increase of area.

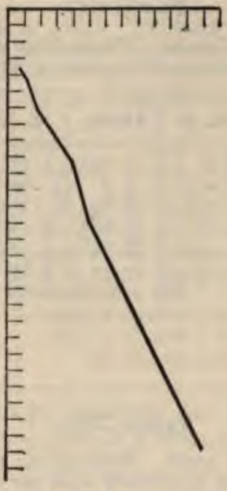
Area	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
3x 3 mm.	4.683	1.26	2.28	.633	2.05	6.963
6x 6 "	3.66	.686	2.26	.626	1.619	5.92
10x10 "	2.876	.557	2.33	.495	1.234	5.206
16x16 "	2.521	.643	2.57	.556	.980	5.091
20x20 "	2.23	.709	2.80	.668	.796	5.030
4x 4 cm.	2.08	.625	2.98	.565	.697	5.06
8x 8 "	2.288	.594	3.805	1.047	.600	6.093
12x12 "	2.03	.523	5.361	1.230	.378	7.391
16x16 "	1.7	.514	12.9	2.385	.131	14.600
18x18 "	1.5	No reappearance.				

The averages of the visibilities and invisibilities of Tables X, XI, XII, and XIII were plotted up to the area 6 cm. x 6 cm., the last reappearance recorded for *F* and *Ge*.



CURVE VIII.

Curve for visibility. Averaged from Tables X, XI, XII, and XIII. *Showing decrease of visibility with increase of area.*



CURVE IX.

Curve for invisibility. Averaged from Tables X, XI, XII, XIII. *Showing increase of invisibility with increase of area.*

It will be noticed, in the above Tables, that the area at which fluctuation ceases has been decreased to one-fourth in the one case and to one-eighth in the other. This, we believe, is due solely to the inequality in intensity of the stimulus obtained by the former method, for it will be observed that the area at which the stimulus began to recur in parts in the former Tables nearly coincides with that at which fluctuation ceased in the latter.

It will be seen, also, that in the case of the smaller areas the phases of visibility have been decreased and the phases of invisibility increased. Sufficient explanation for this result can be found, most probably, in the different conditions for adaptation present in the two cases. It will be well, at least, to point them out.

(*a*) Although of no greater intensity, the stimulus area was more sharply defined than the area given by the reflected light. The latter was somewhat diffuse and spreading, and to a certain degree gave the effect of a larger area. This slight change would be appreciable for the smaller areas, but not for the larger. (*b*) The side of the opal glass used for the background in the former case was polished and shining. This was trying to the eyes of the observer, the strain relieving itself in increased eye-movement and blinking. The side used in the latter case was dull and chalky, and produced no particular discomfort. (*c*) In the former case, the minimal difference to be adapted out was between a bright white background and a still brighter stimulus. In the latter case, it was between a dull, chalky background and a darker stimulus. Just what effect this difference would have on adaptation we are not able to state. It seems reasonable to believe, however, that the process would not be uniform at all points in the white-black series. In fact records were obtained indicating that, in general, this is true; unfortunately, however, they were made early in the work, and were not arranged for a particular confirmation of results under these precise conditions. As nearly as can be determined from them, planned as they were, the process is more rapid at the extremes of the white-black series (indicated by the shorter phases of visibility here obtained) than in the mid-region of neutral grays. The stimulus just noticeably lighter than a black background, too, seems to give slightly shorter phases of visibility than a stimulus just noticeably darker than a white background. It will be understood that these results are not intended to apply to adaptation any further than for the obliteration of just noticeable differences. But from the data we may draw the very general conclusion that the value of just noticeable differences for adaptation, even in the white-black series, depends upon the sort of combination used.

It is important to note that while Lange[1] finds approximate equality in the periods obtained from three sense departments, and argues therefrom a central origin, it is found here that a change of conditions so slight as to have passed unnoticed, had not the results demanded investigation, brings a wide range of variability, although not even a change in the retinal elements stimulated is involved, *i. e.*, both series of combinations of stimulus and background are in the white-black series.

Another point noticeable in these records, and throughout the work in general, is the large mean variation. This is especially obvious when large areas are used, or whenever from any cause either visibility or invisibility approaches infinite value. It is due, chiefly, to one or two very long phases of visibility or very short phases of invisibility, or conversely; the phenomena depending upon which extreme of the phase variation one is considering. Slaughter[2] believes that there is a connection between these recurring long phases of visibility, obtained with stimuli of the usual order, and the Traube-Hering waves. It seems, however, much more probable that their immediate condition is to be found in eye-movement. In unsteady fixation, the eye oscillates, *i. e.*, in recovering fixation,

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[1] *Philosophische Studien*, IV, 390.

[2] *Op. cit.*

TABLE XIV.

Ga. Method of variation of areas. Adaptation: showing inverse variation of visibility and invisibility with increase of area.

Stimulus	Background	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Red, 1x 5 mm.	Blue	No disappearance.					
" 2x 5 "	"	4.64	.98	1.175	.325	3.95	5.815
" 5x 5 "	"	2.77	.601	4.19	1.16	.66	6.96
" 10x10 "	"	1.4	.34	4.44	1.24	.31	5.84
" 2x35 "	"	1.68	.58	3.35	1.09	.5	5.03
Hering Gray, (no. 27) 1x 5 mm.	Hering Gray, (no. 33)	No disappearance.					
" 2x 5 "	"	27.915	1.423	.54	.223	51.694	28.455
" 5x 5 "	"	9.450	2.617	1.568	.57	6.057	11.018
" 10x10 "	"	6.787	2.25	2.275	.325	2.983	9.062
" 2x35 "	"	10.36	3.07	1.485	.285	6.976	11.846

TABLE XV.

E. Method of variation of areas. Adaptation: showing inverse variation of visibility and invisibility with increase of area.

Stimulus	Background	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Red, 1x 5 mm.	Blue	9.5627	2.66	1.075	.175	8.895	10.637
" 3x 5 "	"	4.237	1.07	1.381	.281	3.067	5.618
" 7x10 "	"	1.878	.255	2.863	.594	.656	4.741
" 1x 5 "	Gray	3.542	1.071	10.471	2.514	.3383	14.013
" 5x 5 "	"	2.06	.480	8.55	1.71	.2409	10.61

F. ADAPTATION IS RENDERED INTERMITTENT CHIEFLY BY EYE-MOVEMENT.

"Even in fixation intended to be constant, as in the present investigation, it is not likely that the eye was motionless for the eight to thirty seconds during which the experiment lasted, as McAllister has recently pointed out that the eye is seldom at rest for one-ninth of a second continuously. At least it would be most unlikely that it should be absolutely at rest for so long a period as twenty seconds and then move unconsciously at the end of that time."[1]

The inference contained in the above quotation is that, if eye-movement causes relief of the adapted elements sufficient to bring about reappearance when complete adaptation has once set in, disappearance should never have occurred;

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[1] W. B. Pillsbury: *The Journal of Philosophy, Psychology, and Scientific Methods*, II, 272. Review of *Zur experimentellen Kritik der Theorie der Aufmerksamkeitsschwankungen*, by B. Hammer.

for the eye is moving almost continuously, and each movement should have relieved the adaptation that had taken place previous to it. Pillsbury has, however, probably not considered that range of movement is a factor as well as frequency. If the eye moved nine times a second with sufficient range, there probably never would be noticeable adaptation for small areas. How far this supposition is from the facts, however, is shown by our results. The average interval between movements extensive enough to produce a noticeable shift of the after-image in either the horizontal or vertical plane, viewed at a distance of 1 meter (and certainly smaller movements could scarcely be considered to bear upon the point in question), ranges from $\frac{4}{5}$ sec.-$2\frac{1}{4}$ sec. The average time between movements shifting the after-image 2 mm. in either plane ranges from $1\frac{1}{2}$ sec.-$2\frac{1}{2}$ sec.; 4 mm., from $2\frac{1}{7}$ sec.-48 sec.; 6 mm., from $3\frac{1}{8}$ sec.-96 sec., etc. Now it will be remembered that the movements in each plane came in groups of two and three, so that these intervals in most cases should be so much multiplied. According to this account, there seems to be ample opportunity for adaptation to take place, when range of movement is taken into consideration as well as frequency. Movements as small as those referred to by McAllister would probably produce some effect in delaying adaptation; but complete restoration before the stimulus has adapted out, or reappearance after it has disappeared, is doubtless caused by groups of movements of considerable range.

The range of movement required will, of course, depend upon the stimulus area used. When the area is very small, Pillsbury's inference holds; there is no disappearance. The restoration afforded by eye-movement here cancels the effect of adaptation before disappearance takes place. This is one of the points brought out by our method of areas. On the other hand, with areas varying from 10 cm. x 10 cm. — 14 cm. x 14 cm., the range of movement for our observers was not great enough ever to produce reappearance. And with still larger areas, extensive voluntary movements did not suffice even to revive the lost sensation.

We do not assert that the statement quoted, considered as a criticism of Hammer's article, is not well grounded. This article is chiefly suggestive. But, on the other hand, it is only fair to remember that it requires positive knowledge to overthrow as well as to establish a theory. Both statement and criticism should, with equal care, be based upon ample investigation. That eye-movement, blinking, etc., interfere with the course of adaptation is not a recently discovered fact, nor is it a closed subject. Local adaptation still presents a fruitful field for research.

With the help of the data submitted in this article, we trust that no intrinsic difficulty will be found in the conception that adaptation is rendered intermittent by eye-movement. Aside from this, too, there remains, further to strengthen the theory, the supplementary factor (not yet specified) which works in conjunction with the relief afforded by a shift of the adapted elements into a region of different stimulation.

For the investigation of eye-movement, a method had to be selected that would not be objectionable to the observers, and would not interfere with the normal course of the phenomenon either mechanically or by way of distraction. The shifting of the negative after-image during fixation afforded a method somewhat rough, but adequate for our purpose. Colored strips, 5 mm. x 40 mm., were used as stimuli. They were pasted on a background of white card-board, with the shorter dimension in the plane in which the eye-movement was to be investigated. The determination of frequency then became merely a matter of recording the appearance of the after-image to the right or left or above or below the stimulus, separate series being made for both planes. For the determination of range of movement, narrow strips of paper of the same brightness as the background were placed successively 2, 4, 6, 8, etc., mm. from the stimulus, and only those movements recorded that caused the after-image to shift to or beyond these strips. The strips were so inconspicuous as not to attract the eye away from the fixation point; still, it was not difficult to judge when the after-image reached, or passed beyond them. They were always used, also, when frequency alone was to be determined, in order that the same conditions might prevail throughout. Some periods were given up wholly to the investigation of eye-movement alone, thus determining the type in general; while again the eye-movement tracing was alternated with the corresponding fluctuation tracing, in order to establish a more immediate connection between the eye-movements in either plane and the phases of visibility and invisibility in that plane. Doubtless, it would have been better to have the eye-movement recorded while the fluctuation was in progress, could this have been done without interfering with the normal course of the phenomenon. As it was, however, enough results were obtained to render conclusions safe as to the type of the observer.

The stimuli in both the eye-movement and fluctuation experiments were of the same dimensions, and were arranged in precisely the same way. The distance of the observer, throughout, was 1 meter. The color of the stimulus was selected with reference to the vividness of the after-image for the particular observer. Milton-Bradley standard green was used for *S*, *Ga*, *Ge*, while red of the same make gave the best results for *F*. The expression: 'stimulus vertical' will be used when the longer dimension of the strip is placed in the vertical plane, and 'stimulus horizontal' for the corresponding arrangement in the horizontal plane.

The following results were obtained:

(a) *Eye-movement in the horizontal and vertical planes.*

S. Length of observation: 97 sec.

Stimulus vertical.

Strips 2 mm. distant. Recorded all,	55
All reaching to strips 2 mm. distant,	26
“ “ “ “ 4 “ “	11
“ “ “ “ 6 “ “	4

Stimulus horizontal.

Strips 2 mm. distant. Recorded all,	39
All reaching to strips 2 mm. distant,	21
“ “ “ “ 4 “ “	9
“ “ “ “ 6 “ “	2

The results here show greater range and greater frequency in the horizontal plane. The records also demonstrated that the recovery was quicker in this plane.

Ge. Length of observation: 94 sec.

Stimulus vertical.

Strips 2 mm. distant. Recorded all,	46
All reaching to strip 4 mm. distant,	40
“ “ “ “ 6 “ “	14
“ “ “ “ 8 “ “	7
“ “ “ “ 10 “ “	2

Stimulus horizontal.

Strips 2 mm. distant. Recorded all,	30
All reaching to strip 2 mm. distant,	25
“ “ “ “ 4 “ “	14
“ “ “ “ 6 “ “	0

It will be noticed that the excess of range in the horizontal plane in this Table is considerably greater than the excess of frequency. There is quicker recovery also in the horizontal plane.

Ga. Length of observation: 96 sec.

Stimulus vertical.

Strips 2 mm. distant. Recorded all,	35
All reaching to strips 2 mm. distant,	31
“ “ “ “ 4 “ “	2
“ “ “ “ 5 “ “	1

Stimulus horizontal.

Strips 2 mm. distant. Recorded all,	81
All reaching to strips 2 mm. distant,	9
“ “ “ “ 4 “ “	0

This Table shows greater frequency in the vertical and greater range in the horizontal plane. In the next Table the experiments for range were not carried out. The following averages for frequency were obtained:

F. Length of observation, 104 sec.

Stimulus vertical,	29
" horizontal,	17

Towards the end of the hour, for each observer, the records showed increase of eye-movement, as the result of fatigue.

(b) *Fluctuation with vertical and horizontal arrangement of the stimulus.*

The following are the results obtained for the fluctuation experiments. The stimulus was rendered liminal by being placed behind a plate of opal glass.

TABLE XVI.

S. Fluctuation with vertical and horizontal arrangement of the stimulus. Showing how arrangements that favor maximal and minimal interference with adaptation affect the phases of visibility and invisibility.

Stimulus	Arrangement	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Gray, 5x40 mm.	Vertical	5.3545	1.027	2.4727	.690	2.165	7.8272
" " "	Horizontal	3.0733	.426	2.7066	.666	1.135	5.7799
Red, " "	Vertical	3.152	.917	1.641	.453	1.9207	4.793
" " "	Horizontal	2.358	.452	2.436	.621	.968	4.794
Green, " "	Vertical	4.986	1.06	1.493	.413	3.340	6.479
" " "	Horizontal	3.260	.526	2.546	.786	1.2804	5.806
Yellow," "	Vertical	5.0611	.911	1.588	.511	3.187	6.6491
" " "	Horizontal	4.642	.580	2.542	.371	1.825	7.1856

TABLE XVII.

Ge. Fluctuation with vertical and horizontal arrangement of the stimulus. Showing how arrangements that favor maximal and minimal interference with adaptation affect the phases of visibility and invisibility.

Stimulus	Arrangement	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Gray, 5x30 mm.	Vertical	3.873	.873	2.32	.933	1.669	6.193
" " "	Horizontal	3.825	.983	3.458	1.075	1.135	7.283
" 5x40 "	Vertical	4.006	1.293	2.526	.733	1.585	6.533
" " "	Horizontal	1.407	.268	4.423	1.153	.3181	5.830
" 5x50 "	Vertical	4.353	.822	2.700	.868	1.6123	7.053
" " "	Horizontal	2.700	1.071	3.285	.778	.8217	5.985
Green, 5x40 "	Vertical	4.400	1.054	4.981	1.172	.8833	9.381
" " "	Horizontal	2.663	.490	5.481	1.154	.4857	8.144

TABLE XVIII.

Ga. Fluctuation with vertical and horizontal arrangement of the stimulus. Showing how arrangements that favor maximal and minimal interference with adaptation affect the phases of visibility and invisibility.

Stimulus	Arrangement	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Gray, 10x50 mm.	Vertical	2.655	.644	6.422	1.333	.4135	9.077
" " "	Horizontal	3.336	.763	5.545	1.418	.6016	8.881
Yellow, 2x40 "	Vertical	3.807	1.115	3.223	.915	1.181	7.030
" " "	Horizontal	6.96	1.987	2.45	.801	2.8408	9.41

TABLE XIX.

F. Fluctuation with vertical and horizontal arrangement of the stimulus. Showing how arrangements that favor maximal and minimal interference with adaptation affect the phases of visibility and invisibility.

Stimulus	Arrangement	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Red, 5x40 mm.	Vertical	4.8685	1.29	3.495	.775	1.392	8.3635
" " "	Horizontal	3.038	.646	3.753	1.092	.809	6.791

(c) *Adaptation with vertical and horizontal arrangement of the stimulus.*

That the same arrangement is effective with stimuli at full intensity was confirmed by experiments upon *Ga.* A strip of Hering red, 5 mm. x 30 mm., was pasted on a square of Hering light blue, 20 cm. x 20 cm., and viewed at a distance of 2 meters.

TABLE XX.

Ga. Adaptation with vertical and horizontal arrangement of the stimulus. Showing the interference caused by the vertical and horizontal arrangements.

Stimulus	Arrangement	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Red, 5x30 mm.	Vertical	2.24	.671	4.014	.914	.558	6.254
" " "	Horizontal	3.987	.727	3.775	.887	1.056	7.762

G. CORRESPONDENCE OF FLUCTUATION WITH ADAPTATION IN INDIRECT VISION.

(a) *Fluctuation.* In the fluctuation experiments in indirect vision, the stimuli were rendered liminal by the use of the opal glass plate, as before. The observer was seated at a distance of 1 meter and given a fixation point in the median line. It will be noticed in these results, as also in the Tables for the method of variation of areas, that the average phase of visibility increases slightly at the end of the Table. The reason is that in each tracing the phase of visibility is greatest at the beginning and decreases considerably towards the end. Now in the last series of the Table there are few and, at the very last, no phases of visibility to average with these maximal first phases; consequently the curve rises a little at the lower end. For the same reason, the mean variation for both visibility and invisibility increases towards the end of the Table.

The results obtained are given in the following Tables. The points to be noticed are the effects of variation of area and passage of stimulus towards the periphery.

TABLE XXI.

Ga. Correspondence of fluctuation with adaptation in indirect vision. Fluctuation: showing the effect of increase of area and passage of stimulus towards the periphery.

Stimulus	Distance from fixation	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Gray, 8x8 mm.	0 cm.	2.335	.67	2.38	.485	.985	4.715
" " "	4 "	1.527	.383	3.52	.993	.434	5.047
" " "	8 "	1.13	.345	7.35	2.212	.153	8.48
" " "	12 "	1.08	.39	8.73	2.157	.123	9.81
" " "	20 "	.566	.1	13.38	4.8	.0423	13.946
" " "	24 "	.633	.06	34.53	12.	.018	35.163
" 6x6 cm.	0	1.49	.64	6.33	1.73	.235	7.82
" " "	4 "	1.177	.355	8.411	2.07	.140	9.588
" " "	8 "	.628	.214	10.685	2.514	.0588	11.312
" " "	12 "	.625	.175	23.125	3.875	.0265	23.75
" " "	20 "	.250	.03	43.25	7.282	.0057	43.75
" " "	24 "	.2	No reappearance.				

TABLE XXII.

F. Correspondence of fluctuation with adaptation in indirect vision. Fluctuation: showing the effect of increase of area and passage of the stimulus towards the periphery.

Stimulus	Distance from fixation	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Gray, 5x5 mm,	0 cm.	4.12	.83	2.41	.50	1.709	6.53
" " "	8 "	1.671	.507	4.514	.102	.370	6.185
" " "	12 "	1.187	.5	9.625	2.28	.122	10.812
" " "	20 "	.433	.123	22.25	4.82	.0194	22.683
" 6x6 cm.	0 "	2.35	.46	4.77	1.13	.492	7.12
" " "	8 "	2.24	.65	14.04	3.158	.1592	16.28
" " "	20 "	1.5	No reappearance.				

TABLE XXIII.

Ge. Correspondence of fluctuation with adaptation in indirect vision. Fluctuation: showing the effect of increase of area and passage of stimulus towards the periphery.

Stimulus	Distance from fixation	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Gray, 6x6 mm.	0 cm.	3.975	1.21	4.187	1.33	.949	8.162
“ “ “	8 “	2.1875	1.1	8.4	.24	.2604	10.5875
“ “ “	12 “	1.128	.285	1.528	3.045	.09785	12.656
“ “ “	20 “	.79	.25	11.04	.665	.0715	11.83
“ “ “	28 “	.5	.21	11.728	2.971	.0426	12.228
“ “ “	34 “	.375	.1	11.987	2.446	.0312	12.362
“ “ “	43 “	.5	.2	44.9	9.62	.0111	45.4
“ 6x6 cm.	0 “	.62	.22	22.75	4.64	.0272	22.97
“ “ “	8 “	.7	.2	42.6	9.89	.0151	43.3
“ “ “	12 “	1.	No reappearance.				

(b) *Adaptation.* That stimuli at full intensity show the same law of inverse variation of visibility: invisibility from direct vision towards the periphery was verified by *Ga.* Hering standard red upon a background of engine-gray card-board (neutral shade) was used.

TABLE XXIV.

Ga. Correspondence of fluctuation with adaptation in indirect vision. Adaptation: showing the effect of passage of stimulus towards the periphery.

Stimulus	Distance from fixation	Vis.	M. V.	Invis.	M. V.	Vis.: Invis.	Period
Red, 8x8 mm.	8 cm.	15.916	3.25	.2	.033	79.58	16.116
“ “ “	12 “	6.575	2.05	1.683	.475	3.907	8.258
“ “ “	20 “	4.293	1.13	2.656	.925	1.612	6.949
“ “ “	24 “	4.89	1.05	3.35	1.151	1.46	8.24
“ “ “	34 “	3.2	1.192	5.5	1.107	.581	8.7
“ “ “	42 “	2.9	.766	6.583	1.208	.440	9.73
“ “ “	50 “	2.33	.86	9.26	2.32	.240	11.56
“ “ “	60 “	.45	No reappearance.				

The adaptation times in indirect vision were also obtained for the same stimulus. The background in this case was, as before, the neutral engine-gray card-board. The time required completely to adapt out the stimulus was recorded. This, an adaptation experiment in its purest form, shows that the time required to adapt out the stimulus decreases as we go towards the periphery. One may suggest the following reasons why this decrease should occur:

(1) Decrease of the retinal stuff towards the periphery. This would certainly be true for colored stimuli.

(2) Since the eye is approximately spherical in form, and the aperture is near the front surface, one might expect less absolute change of stimulation area towards the periphery on account of eye-movement. Experiments to test the matter, by the same method as was used in direct vision, showed a marked decrease in the number of eye-movements recorded as the stimulus was moved towards the periphery. Whether this was because there was actually less range of movement of the after-image, or was due solely to the greater difficulty of observation, we are not able to state. The fact that there was a greater decrease in range than in frequency would seem to indicate that the effect was not wholly due to increased difficulty of observation.

(3) A further reason will be discussed when we deal with the fluctuation of after-images.

The observer sat with eyes closed and registration key up. The drum was started. At a signal the observation was begun and the key pressed down. When the color had adapted out, the key was released. The results were as follows:

Ga. Time unit: 1 sec.

Stimulus 8 cm. from fixation,	15.8
" 12 " " "	9.7
" 20 " " "	5.0
" 28 " " "	4.8
" 54 " " "	2.8

II. Cutaneous Stimuli.

(a) *Pressure.* Liminal pressure stimuli were applied to several observers, but no fluctuations were experienced. Very smooth cork wafers supporting minimal weights were used, and every care was taken to insure uniformly distributed, pure pressure sensations.

(b) *Electro-cutaneous.* Liminal electro-cutaneous stimulation was also tried. The tip of the tongue was selected as the area most sensitive to stimulation. Strips of very light tin foil (Christmas-tree foil) were used as electrodes. The moist surface of the tongue readily held these in place. There was no preliminary sensation of pressure or contact. The observer was not even able to tell that the strips were in place when the current was off. A Du Bois-Reymond sledge was chosen as giving the most easily regulated induction current. The observer was seated in a distant room, his head fixed in a head-rest, and the electrodes clamped in place. He was thus isolated from all noise and distracting influence. An electric button was near his hand by means of which he could signal to the experimenter and thus regulate the intensity of the stimulus. With care just noticeable stimuli were easily obtained; but no fluctuations of intensity could be detected, although repeated attempts were made on a number of observers. It hardly seems possible that failure to obtain fluctuations could have been due to faulty conditions.

We submit these results hoping that, when they have been verified elsewhere, they will prove as decisive to others as they have been to us. We trust that in them ample evidence has been afforded that Lange advanced the theory of fluctuation of attention upon insufficient data. Indeed, that an attempt ever should have been made to gather together these discrete sense-phenomena under the head of 'fluctuation of attention' seems more the result of doctrinal development than of a thoroughgoing consideration of the phenomena themselves.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE.

COMMUNICATED BY M. F. WASHBURN.

II. A COMPARISON OF METHODS FOR THE DETERMINATION OF IDEATIONAL TYPE.

By ALMA BELL and LORETTA MUCKENHOUPT.

The object of this study was to test the mutual consistency of the various methods given in Titchener's Experimental Psychology, Volume I, Part II, pp. 394 to 401, for investigating the type of imagery normal to a given individual. For a description of these methods the reader is referred to the above mentioned volume. Since only two of them, the revised Galton questionary and Secor's word method, undertake to discover the presence of images other than auditory, visual or motor, for purposes of comparison these types of image only were taken account of. The plan of the investigation was to test a few observers by as many of the methods as possible in order to see whether the results were in harmony with each other. Four women students of psychology, with a half year's practice in introspective work, were the subjects. Two of them, H., and B., were not tested by Kraepelin's method; and one, B., was not tested by Cohn's method or Washburn's method. Otherwise the accompanying table allows a thorough-going comparison of results.

	Observer, Vo.				Observer, H.				Observer, B.				Observer, S.			
METHOD.	V.	A.	M.	Gen.	V.	A.	M.	Gen.	V.	A.	M.	Gen.	V.	A.	M.	Gen.
Kraepelin's,	.69												1.09			
Secor's Visual,	36	33	2½	22	51	15	0	38	60	17	0	22	58	14		
Secor's Auditory,	44	26	0	24	46	28	0	19	48	26	0	12	51	21		
Binet's, part I, a,	10.4	37	0		0	58	0		56	9	2		52	0	16	
Binet's, part I, b,	0	55	0		0	91	0		13	44	11		38	47	0	
Binet's, part II, a,	12	66	33		45	56	62		64	54	16		33	22	16	
Binet's, part II, b,	27	91	33		50	55	47		64	86	22		100	44	33	
Cohn's,	29	58 A.V.			58	43 A.V.							100	95 A.V.		
Washburn's,	11	66	22		0	50	50						20	0	80	

In explanation of the table, the following points should be noted. The results for a given method on all four observers, it will be seen, are to be found in the same horizontal column. The vertical sections of the table belong respectively to the different observers. The vertical columns in each section marked V., A., M., Gen., contain figures representing the value of visual, auditory, motor, and other miscellaneous images for each observer, determined by each method. The figures under V, opposite Kraepelin's method, represent the ratio of the number of objects characterized by color to that of objects characterized by sound, thought of by the observer in five minutes interval. Obviously this ratio, and not the absolute number of ideas occurring to the mind, is the proper representative of ideational type, since the absolute number of ideas depends on the rapidity of the flow of ideas, which may vary quite independently of their type. The figures opposite "Secor's Visual" and "Secor's Auditory" are the percentages of visual, auditory, motor and other images suggested to the different observers, in the one case by the list of words read visually, in the other case by that heard read aloud. Opposite "Binet's, Part I, a," are given the results for this method reduced from "forty-eighths" to percentages for the purpose of better comparison. All the results for the various parts of Binet's method and for Cohn's method are similarly stated in percentages. It will be seen that no figures are given for section (c) in either part of Binet's method. This section, as a reference to the Manual will show, tests chiefly the motor memory by means of nonsense figures. Its omission in our table is due to an error in method. The four observers should have been tested by the same figures; instead of which each of the two experimenters made her own figures, which were considerably more difficult in one case than in the other, so that the results were not fairly comparable. The table as a whole is of value, however, only for the comparison of auditory and visual types, since two of the methods, Kraepelin's and Washburn's, do not attempt to test any other form of image; and this value is little affected by the omission of the nonsense figure experiments. Since the second part of Cohn's method tests the composite auditory-motor type of memory, the percentage figures here are placed between the auditory and motor columns. The results of Washburn's method need a few words of further explanation. This method supposes that it is possible to obtain an objective means of deciding whether a given mistake in addition has involved visual or auditory images. The criterion suggested in the Manual involves putting down as auditory all mistakes where a figure is repeated, as for instance, "282+20=303; 569+23=593." Our experience suggests that these mistakes may perfectly well be visual; it is quite as possible to confuse the visual images of the numbers in such a way that a number is repeated, as it is to repeat through 'sound echo.' Guided by our own introspection and that of our observers, we adopted the following method of distinguishing between auditory and visual mistakes. (1) Those errors involving the repetition of a figure whose sound was not the same in the two cases were put down as visual, for instance, "193+16=213;" "245+17=252." Obviously there can be no question of a sound echo here. (2) Errors involving the repetition of a number further back in the series than the two added were put down as auditory, for instance, the repetition of 8 in the following: "143+8=151, 151+4=158;" or of 7 in "542+7=549, 549+15=574." The reason for calling these auditory is the introspective one that even good visualizers do not retain the visual images of numbers any further back than those which they are actually adding, while the sound echo of a number may persist for some little time. (3) Errors involving the repetition of one of the numbers added, where the sound as well as the look of the number remained the same, were put down as auditory-visual, since they might have been due with equal probability to the persistence of visual images or to that of sound images. The results given for each observer are thus the percentages of the total numbers of errors made falling under each one of these three heads: visual, auditory, auditory-visual.

The following conclusions may be drawn from the table:

(1) The methods are, generally speaking, consistent in their results.

Observer Vo. appears to be the most auditory and least visual in type of the four observers. H. comes next, while B. and S. are predominantly visual. It will be observed that Vo. shows a predominance of the auditory over the visual memory by every method except Secor's; that H. shows a similar predominance by every method except Secor's and Cohn's; S. shows a superiority of visual over auditory memory by every method except Binet's, Part I, b; and B. a like superiority by all the methods used on her except Binet, Part I, b, and Binet, Part II, b. As regards these exceptions, we may note that Part I, b, of Binet's method gives the greatest preponderance of auditory over visual memory for all four observers; in the cases of Vo. and H. it is the only one where the visual memory absolutely touches zero. The mode of procedure in this method is for the experimenter to read aloud to the observer series of nine letters each; at the end of each series the observer recalls it and dictates it to the experimenter. No effort is made to control the kind of memory involved. Under these circumstances, then, all four of our observers found it more natural to be guided by the persistent sound image of the letter than by its imagined look, which is not to be wondered at when we remember that the sound images are occasioned by the actual utterance of the sounds, while the visual images are merely suggested both in the recall of the series and in its first presentation. The difference in type between our four observers is evidenced by the fact that Vo. and H. had no visual imagery involved, while B. had a small amount and S. nearly as much visual as auditory imagery. An important difference exists between Parts I and II of Binet's method which affects comparison of their results. The figures under V. and H. in the case of Part I represent the natural uncontrolled tendency of the mind under circumstances favoring, for (a), visual memory, and for (b), auditory memory. The figures similarly placed in the case of Part II represent the results of an effort to recall in visual or in auditory terms, the conditions favoring as before (a) visual and (b) auditory images. All the observers seem to have visualized better when they made an effort to do so; the effect of intentional use of auditory images is not so marked. It is a curious fact that three observers succeeded better at deliberate visualizing in Part II (b), where the conditions favor auditory imagery, than in (a) where visualization is favored; while the fourth observer, B., did as well in one case as in the other. Probably the fact that memorizing a nine-letter series is an easier problem than memorizing a twelve-letter square helped to produce this result.

(2) The method which in this test gave least satisfactory results was Secor's. The series of words suggested in the Manual, like the others used in our experiments, does not offer equal opportunities for visual and for auditory associations, and the results could not be expected to be trustworthy as an absolute measure of an individual's mental type; however if, as was here the case, the same series is used on different individuals, it should be possible to obtain a comparison of the tendencies of these individuals. It will be seen from the table, however, that the four observers showed less difference in the results of tests by this method than in those made by any of the other methods. In "Secor's Visual," observer Vo. displays her characteristic auditory tendency in the ratio V : H :: 36 : 33, where all three of the other observers show much greater preponderance of visual over auditory images. But in "Secor's Auditory" the results from all four observers are very nearly alike. The root of the difficulty with Secor's method, the source of its defects, lies probably in the fact that it requires the exercise of perfectly indefinite and uncontrolled processes of association. All the other methods demand of the observer the revival of certain fixed and specified material,

letters, numbers, etc.; the variations in the results are due wholly to the mode of revival. But where words are used and allowed to suggest images freely, the process is bound to be complicated by all the influences which determine the play of associations. The recent experience of the observers will be a disturbing factor; if one has just seen a picture of a conflagration the word 'fire' may suggest the picture, although, under ordinary circumstances, auditory imagery would be called up. Then the nature of the imagery suggested by one word may affect that suggested by the next: if 'fire' calls up a visual picture the following word may also suggest a visual image; if the idea called up by 'fire' be auditory the idea called up by the next word may be auditory, too, and so on.

Finally, it is interesting to compare the results of these relatively objective methods with the introspective testimony of the four observers as to their mental type, given in answers to the questionary on pp. 198-200 of the Students' Manual (Vol. I). Observer S., one of the good visualizers, judged by the results in the table, declares herself able to call up the picture of the rosebuds, etc., very vividly, and to the question, "Is the image as bright as the objects would be if they lay on the table before you?" answers, "Yes, of course." B., the other good visualizer, says, "I think the image is as bright as the objects themselves would be." Both S. and B. place the visual experiences first in order of vividness among those mentioned on page 200. Both say that they do not recall music easily and that imagined music does not play any considerable part in their mental life. H., less visual and more auditory by the other tests, says of the imaged rosebuds, "At times the image is as bright as if the flowers were before me, but it becomes blurred in a very short time." She also states that if she fixes her attention on one part of the image the other parts become blurred. B. says, "no part of the image is blurred, though the flowers lying on the ferns are the most prominent part." S. says the image is all clear, though "one side of the box is shaded on the inside." H. says she can recall music easily, and that imagined music is often disturbingly real to her. She can see the words of the national anthem printed, but only when she imagines herself repeating them at the same time. She does not, however, note any marked difference in the vividness of auditory and visual images in the series on page 200. V., the most strongly auditory and least visual of the four, according to the other methods, although she does not find any special defect in her pictures of the rosebuds, places auditory images first and visual images last of all in point of vividness among the experiences named on page 200. In answering the questions on the national anthem, she says, "Effort is necessary to see the words printed." She recalls music easily and has a great deal of musical imagery.

On the whole, then, the questionary results support those of the other methods. One point, however, should be noted. Observer H., though her auditory imagery seemed to be better than her visual imagery by most tests, declares that she uses the latter constantly in her ordinary thinking. This is evidence of the fact that in determining mental type tests of the vividness of a certain kind of image should be distinguished from tests of the frequency of its occurrence. A person may have a mediocre power of visual imagery, measured by the definition and vividness of the pictures called up, and yet may think habitually in visual terms.

THE KINÆSTHETIC ELEMENT IN ENDOPHASIA AND AUDITORY HALLUCINATION.

(From the Psychological Laboratory of the University of Pennsylvania.)

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Since Charcot applied the concept of three memory types, visual, auditory, and motor, to the verbal memory, many investigators have endeavored to analyze the forms in which we think, and to determine more exactly their precise nature.

Binet's wide experience leads him to conclude that all thought is either verbal or sensory. Assuming this to be true, much of our exact thought must be verbal, words being the only possible images of a great number of complex concepts. This thinking in words has been found to be accomplished in various ways by different individuals. Some see, some hear, some write and some say the words to themselves, while still others, probably the majority of persons, combine some or all of these methods.

Verbal thinking or internal language, of any type, is now generally known as endophasia, a name originally suggested by Morselli.

Charcot held that the verbal memory depended upon visual, auditory, articulatory and graphic imaging of words; these types being used independently or in various combinations by different individuals.

Some later investigators have thought the process less complex. Dejerine, for instance, while agreeing with Charcot that all these types enter into the imaging of objects, refuses to apply them all to verbal thinking, holding that the latter partakes of two and only two forms of imagery—the auditory and the articulatory. Egger goes still further and limits the process to one type, the auditory. Still later experimental investigations, however, have brought forth evidence in support of the older theory of Charcot.

Lemaitre, in 1904, published the result of his observations on ninety school children, varying in age from thirteen to fifteen years. Among these forty-five per cent. were purely motor in their verbal thought, thirteen per cent. were purely auditory and fourteen per cent. purely visual, while the remainder variously combined the types.

G. St. Paul published in the same year the results of an extensive investigation of the subject by the questionnaire method. Two hundred and forty persons answered his questions. Thirty-eight failed to give clear information; of the remaining two hundred and two, forty-eight per cent. were auditory-motor in type, twenty per cent. were visual-motor, fifteen per cent. auditory, seven per cent. motor, nine per cent. visual and one per cent. auditory.

Both Lemaitre and St. Paul found all three types to exist, though in quite different proportions. In a large percentage of the subjects of both investigators, the articulatory images are prominent. They appear in seventy-five per cent. of St. Paul's subjects, and in forty-eight per cent. of Lemaitre's.

With such experimental evidence we seem justified in assuming that the articulatory element plays an important part in endophasia. When this articulatory element predominates, the internal language assumes the character of internal speech, —the subjects only secondarily hear or see the words, they say them to themselves.

During the process of internal speech the kinæsthetic speech centres are in a state of excitation which would lead, if uninhibited, to actual vocalization. Other brain processes, however, active at the time, inhibit the motor fulfillment either partially or entirely, the result being internal speech, accompanied or not by some slight movement of the vocal organs. The natural tendency of internal speech is toward actual vocalization, or rather internal speech is actual speech, partially inhibited.

As a rule in the healthy adult the mental content is very complex, and the accompanying brain processes equally so. There is therefore sufficient opposition from various brain centres to inhibit the motor tendency of internal speech. Occasionally, however, when a thought practically absorbs the attention, it is involuntarily given utterance. We are sometimes surprised when alone to find ourselves expressing some thought aloud. This tendency toward vocal expression is noticeable in the majority of persons when reading to themselves—there is usually a more or less pronounced movement of the lips.

It is very strong in children when they begin to read and to work out little arithmetical problems. It is sometimes almost impossible to make them read and count to themselves. This is equally true of adults whose ability to read has not progressed much further. Their lips always move in reading and usually the words are actually spoken. In such cases the mental effort is great enough to absorb the whole attention and the uninhibited motor result is sure to follow.

With some persons the internal speech habit takes the form of soliloquy. This verbal thought is a monologue, a revery. With others, on the contrary, it is usually conversational in character. The thoughts follow one another in dialogue form. Their minds are debating fields and new thoughts are born of contradictions. This conversational form of internal speech may again be divided into two classes. In some cases the dialogue is impersonal, both parts being equally the words and thoughts of the thinker. In other cases the second person of the dialogue is personified, and the individual imagines himself talking to some one else, and endeavors to argue as that other person would under the circumstances.

In this latter type the part of the second person is often largely auditory in character. Thinking of the words as coming from another, one would naturally associate them with the tones of that person's voice, as one's consciousness of another person's speech is, as a rule, almost entirely dependent upon auditory impressions. As our perception of our own speech is largely based upon articulatory sensations, the auditory element is not so likely to be prominent in the personal part of the dialogue.

The greater the predominance of the articulatory factor the greater is the tendency toward actual vocalization; and we find that in types like that just described, the part of the first person is much more apt to be vocalized than that of the second.

When this perfectly normal tendency to internal speech exists in the insane the usual inhibitory influence of a complex consciousness is lacking and various characteristic phenomena result. One woman who is sane enough to realize and analyze her condition to a certain extent, tells me that when she is thinking "the words come out of themselves," she has no intention of saying them and seems to have no control over them. She adds that when she is eating she "seems to be chewing words." In her case, the inhibitory control of opposing centres is greatly reduced, and verbal thought tends strongly toward vocalization. There is, however, enough inhibitory power remaining to awaken in the subject a sense of its inadequacy and of the impulse character of her speech.

Some insane conditions which are apparently dependent upon the tendency to internal speech, I shall describe as they appear in several patients at the Friends' Asylum, Frankford.

The first case illustrates the habit of internal verbal soliloquy. The patient continually carries on a low, almost inaudible talk. One might think she was conversing with some one, but, on listening, I found the talk to be as follows: "A pretty dress, yes, yes, a pretty brown dress, yes, yes, yes, a pin, yes, yes, a bracelet, yes, yes, a chain, yes, yes, etc." This was all in description of a person at whom she was looking. I told her to talk louder, she said: "Talk louder, louder, louder, talk louder, louder, louder." Then I said, "do not talk so low," and the echo came "Do not talk so low, so low, so low, low, low."

Every perception, every thought, evidently finds expression in speech. Speech is no longer used by her as a means of communication with others, but is the involuntary result of all perceptions, all thought.

With the second patient the internal speech habit was evidently of dialogue form, and impersonal in character. This woman hears hallucinatory voices continually. Every creak of a door, tick of a clock, sigh of the wind, or twitter of a bird was laden with a message for her. Her fingers, feet and all parts of her body talked to her, as did also pictures and hallucinatory visions.

The voices were continuous, and were judged as coming from anything on which her attention was, for the moment, centered; whether this was a sound, a visible object or her own body seemed immaterial. She argued with the voices, which took up her thoughts and questioned her about them. She relates, for instance, that one day when she was wondering whether she had a spiritual body, a bird suddenly broke in with "What is a spiritual body, speak quickly, speak quickly." At times she had a dim fancy that in spite of their seeming reality, these voices might be her own thoughts, but the idea was at once stifled by the knowledge that the thoughts expressed were quite unlike hers in character.

Her type seems to be largely auditory as vocalization was rare and never occurred in the second person. The hallucinatory condition was preceded by a constant ringing in the ears, which still persists, though the patient no longer hears the voices and is rapidly regaining her normal condition.

Another patient listened intently to a conversation between several persons, without vocalizing in the least, only showing by her rapt attention and her impatient requests of those around for silence, that she was experiencing an hallucination. When it was over she explained to me what it was all about, and believed in it thoroughly. Here, then, is an example of the personal dialogue form, extended to include many personalities, while the experiencing individual plays the part of spectator and critic. As in the preceding case the type is largely auditory and the voices partake of an hallucinatory character.

The fourth case illustrates the habit of internal conversation in which the part of the second person is personified, though not hallucinatory in character, and in which the part of the first person is predominantly motor in type. I found this patient one day, standing talking most animatedly to no one. She would apparently listen to what some one had to say and then answer at great length. On questioning, however, I found that she was only making believe that some one was there, and talking as she would if that were the case. It was simply the child's game of make believe. She evidently thought internally the responses to her talk, but she externalized only her own individual part of the conversation.

The fifth case is similar to the last, with one exception. The part of the second person, though not vocalized, is of an hallucinatory character. The patient feels sure that she hears others talking to her, she recognizes the voices as belonging to friends and relatives. She will turn suddenly away when in the midst of a conversation to answer something she alone has heard. She localizes the voices as coming from a given direction. The part of the second person in these conversations is evidently largely auditory, though not purely so, as in times of great agitation and excitement it is also occasionally vocalized.

The sixth case is illustrative of the habit of internal conversation in which both parts of the dialogue are motor in type, though the part of the second person is personified and is not so strongly motor as that of the first person. The part of the second person is, like that of the third case, hallucinatory in character.

This patient continually moves her lips and talks in a whisper. She then is apt to say "I hear" as a prelude to an unending flow of talk. If asked who says these things she stops to think and says—"It seems to be my mother," or perhaps sister, or some friend or sometimes God. She also talks much aloud when alone as though addressing these voices. The personal part in her talk is the more strongly motor in tendency, it is spoken in a loud, rich voice, while the part of the second person is merely murmured and sometimes is only betrayed by a slight movement of the lips. This second part is also probably largely auditory in type, as she localizes it in space, and never fails to sharply distinguish it from her personal thought. What a prominent part these hallucinatory voices play in her mental experience, and how clearly they are defined from what she recognizes as her own thought, is shown by the following test. I asked her to write a list of words. She writes rapidly, and in fifteen minutes had written three hundred and thirty words in sentence form, prefixing to each sheet the words "I hear." I then asked her not to write what she heard, but to write me a list of words which she thought of herself. In the same length of time she wrote forty-one words. She made a visible effort to think of words and would sometimes start one, then pause, saying, "No, I heard that," think a moment, and finish out the word in some other way.

The seventh case is again the result of personified internal dialogue. The old lady sits day after day talking to her sons and daughters, her grand-children and all those whom she has loved in the past. She always answers herself and is sure they are somewhere near, though she does not see them, for "they must be" when she "hears them talk." She thought me quite ridiculous for asking whether they were here. "Of course they are, don't you hear them?" said she. Both parts of the conversation in this case are strongly articulatory in type.

The eighth and last case is also illustrative of the habit of personified internal dialogue, in which both parts are equally motor in character. This case which drew my attention to the character of the whole group, is that of a woman who is harassed continually by "people talking to her." I could hold her attention no longer than a few seconds, when she would turn excitedly away to answer some imaginary voice. "Do make them stop, so that I can talk to you. Do make them stop, they frighten me so. They say such dreadful things. Did you hear that? They are going to murder me," etc. Her speech was so badly affected that it was difficult at first to follow her talk, but I soon found that all she hears she said herself. She would say something, and then answer in a different tone of voice and with very different emotional expression. She would almost weep in her own character and the next instant break out in some angry threat. In this case the motor tendency predominates in both the personal and the hallucinatory portion of the conversation. The voices are not localized at some distance from the patient, as they were in the three preceding cases, when the auditory characteristics were strong, but in her head or mouth. She says that these people, whom she names, use her mind and sometimes her voice, and that she cannot stop them and they drive her frantic.

These eight cases seem to me to illustrate the different varieties of internal speech, isolated, undisguised, and uninhibited, as they never appear in a normal mind. The first case is one of internal soliloquy. The second is one of impersonal external dialogue, in which the auditory element is quite strong, and in which the part of the second person is hallucinatory in character. The third is one of personified internal dialogue, in which, also, the auditory element is strong and the part of the second person is of hallucinatory character. The fourth is one of personified internal dialogue, in which the stronger motor tendency is in the part of the first person. The fifth differs from the fourth only in that the second person is hallucinatory in character. The sixth is similar in form, the motor tendency being strong in both parts of the dialogue, though predominantly so in the part of the first person. The seventh and eighth cases are both examples of personified internal dialogue in which the part of the second person is hallucinatory, and equally motor in type with the part of the first person. In the seventh case, however, it is probable that the auditory element is strong in the part of the second person, while in the eighth case it is weak or altogether lacking.

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THE NEGATIVE ASPECT OF HALLUCINATIONS.

(From the Psychological Laboratory of the University of Pennsylvania.)

By Clara Harrison Town,
Resident Psychologist at the Friends' Asylum for the Insane, Frankford.

In an article on "The Psychology of Hallucinations" published in the Journal of Mental Science, October, 1904, Dr. W. H. B. Stoddard elaborates a theory of hallucinations which is largely based upon the belief, that while an individual is experiencing an hallucination he is incapable of experiencing a sensation or perception of the same modality. He holds that there is no physical difference between the processes of perception, ideation, illusion and hallucination; that they each depend upon an excitation of sensory centres, but that in each case the stimulation of these centres is from a different source. For perception the stimulation is peripheral, for ideation central (while a simultaneous process of perception may be in progress), for illusion it is both central and peripheral, and for hallucinations it is central alone, with absolutely no accompanying peripheral stimulation. With hallucinations, he believes, there is a dissociation of the sensory centres affected from the neurons which, normally, conduct to them stimulation from the sense organs.

Dr. Stoddard bases his theory on an eighteen months study of the subject, during which he found no case where an hallucination was accompanied by perceptions of the same modality. Every hallucinatory process has, he thinks, two sides, a positive side which results in the hallucinatory image and a negative side which prevents simultaneous perception by the same centre.

Bearing Dr. Stoddard's theory in mind, I have carefully studied the phenomena of hallucination as they appear in many patients of the Friends' Asylum for the Insane, Frankford.

I have found that the negative factor is not invariably present. In many cases it is evident, in many more it is most difficult to determine whether it is present or not, but in a few it is unmistakably absent. These few cases I shall describe in detail.

The first case is that of a young girl who is almost continually distracted by voices. I endeavored to hold her attention by reading to her. This I could do but for a moment, when she turned distractedly away to answer some threatening voice. I continued to read, and, later, when her attention returned to me, I asked her whether I had been reading while she was holding this conversation. She answered, "Yes, I heard you read, but I could not understand a word you said; won't you stop their talk, so that I may listen to you?"

The next case is that of a woman who spends most of her time talking to imaginary people, whom she seems to see as well as hear. She has frequently made me a third party in these conversations, answering my questions and referring what she hears to me. At other times, however, I have failed entirely, in spite of repeated efforts, to gain her attention.

Another woman will usually answer my questions when in the midst of a conversation with the unseen, but at times I have spoken to her in a loud voice and even shaken her without effect.

A fourth woman was leaning forward in eager attention one day listening to a conversation, snatches of which she would repeat to me, saying—"Hush, I shall tell you later!" A nurse entered the room making a slight noise, when she said peremptorily—"Will you please be still, that I may hear this conversation."

These four cases are, undoubtedly, true examples of genuine auditory hallucinations. In every case the patients were able to identify the speakers.

They seem to be good evidence that all hallucinations are not negative in character.

In a study of visual hallucinations, the result was the same. Sometimes the negative side was evident, and sometimes the immediate surroundings formed the setting for the hallucinations.

These facts seem to call for some other explanation than that offered by the theory of dissociation of peripheral tracts. An adequate hypothesis must explain equally well an hallucinatory process possessing the negative characteristic and one that does not; and should, moreover, demonstrate why this negative factor is sometimes present and sometimes missing.

Is it not possible to find such an explanation in the variability of the degree and span of attention?

If the visual or auditory centres are in a state of great activity, initiated by central stimulation, it is quite possible that the accompanying ideas or images may absorb the whole attention to the utter exclusion of all other sensations. In such a condition there would be no physiological dissociation, the phenomena would be the result of a concentration and limitation of the attention.

An analogous condition is that of the normal individual when he is engrossed by a book or some line of thought to such an extent that he becomes oblivious to his surroundings. It is sometimes nearly impossible to gain a scholar's attention, and ordinarily intelligent individuals often become so absorbed in a novel that it takes repeated calls to arouse them.

When hallucinations occur it appears to me that this condition is only carried one step further. The isolated idea which in the normal individual is felt to be internal, that is, a part of himself, of his ego, is now thought to be a foreign phenomenon, a part of the non-ego of the external world.

As Stoddard says, every perception of an object and also every idea of an object has certain space relations, and is always projected and localized in space to some extent. In such a mental condition as that above described, the idea, with all its local character, is strong enough to predominate over actual peripheral stimulation arising from the same place in space. Such a projected perception or idea centrally aroused differs not at all in character from a similar perception aroused by peripheral stimulation, and to the experiencing individual it is equally real.

With a normal individual the mental content is so complex that it is usually impossible for one idea to absorb the attention to the extent necessary for the production of hallucinations; a balance is maintained among the many psychic phenomena. When many brain centres are simultaneously active they exercise an inhibitory effect one upon the other and prevent undue activity of any one centre.

With the insane, on the contrary, it is a well recognized fact that the mental life is characterized by a monotonous repetition of some one line of thought. Certain brain centres are constantly active, while others are sluggish or entirely inert.

As attention depends upon the mental content, being strong where this content is rich and vivid, and weak where it is impoverished, such a mental state is favorable to the limitation of the attention to one line of thought and also to great concentration upon it.

If this limitation of the attention is very marked and the hallucinatory idea is strong enough to completely fill its span, the negative character of the phenomenon will be manifest. If, on the other hand, the span of attention is a little more extensive and the hallucinatory idea does not completely fill it, other sensations and perceptions may accompany it.

Hallucinations, then, may depend upon an abnormal activity of certain brain centres coupled with a diminution of activity of the other brain centres, or in psychical terms, on a concentration and limitation of the attention.

An hallucination is, according to this theory, simply a normal mental process in isolation, uninhibited by other mental processes; and the isolation is produced not by dissociation but by a narrowing of the field of attention.

LITERATURE.

Selected Sermons of Jonathan Edwards: Edited with Introduction and Notes by H. N. GARDINER. New York, The Macmillan Co., 1904. pp. xxix, 181. Price, 25c.

This volume of the Macmillan pocket series of American and English Classics contains an introductory account of Edwards by Professor Gardiner (23 pp.), a very careful reprint of seven selected sermons (153 pp.), and an appendix of notes (27 pp.), together with a portrait of Edwards from the painting of 1740, a facsimile of the MS. of the first page of the sermon on Spiritual Light, and a cut of the Meeting-house at Northampton in which Edwards preached. The sermons are those on Man's Dependence and on Spiritual Light, the Enfield and the Farewell sermons—these chosen for intrinsic reasons; that on Ruth's Resolution, selected as a revival discourse of a different type from the Enfield sermon; that on A Strong Rod Broken and Withered, which gives Edwards' picture of the Christian Statesman; and that on Many Mansions, which has not before been published. The editorial work has been excellently done. It would be an improvement if the page-headings were made analytic; and the final blank page might be utilized, in another edition, for a brief bibliography. E. B. T.

Hume: The Relation of the Treatise of Human Nature, Book i, to the Inquiry Concerning Human Understanding. By W. B. ELKIN. New York, The Macmillan Co., 1904. pp. ix, 330.

"There is a general impression," writes Professor Elkin, "that the position which Hume adopted in the Inquiry is not identical with that which he had previously assumed in the Treatise, and consequently, that the philosophical principles of the latter work are not exactly the same as those of the earlier. . . . On the other hand, some writers assert that the position or standpoint of both books is essentially the same. But then, regarding the exact nature of that position there are again differences of opinion. . . . To clear up this obscurity on the question of Hume's exact position in his two chief philosophical works is the aim of the present investigation." The comparison has been minutely and impartially made, with the result (given in detail, pp. 293 ff.) that the two works represent essentially the same standpoint, the principal differences in the Inquiry consisting of omissions and additions. It is a pleasure to notice that Dr. Elkin avoids the fallacy, all too common in philosophical writings, of making Hume a modern psychologist or identifying modern psychology with Humian psychology. The book should have had an index. E. B. T.

Religion: its Origin and Forms. By J. A. MACCULLOCH. London, J. M. Dent & Co., 1904. pp. 185.

This volume of the Temple Primers opens with an introductory sketch of the province, problems and history of the science of religion. Then follows a group of chapters dealing with the origin of religion, under the headings Animism, Nature-worship, Animal-worship and Totemism, Ghost-worship, Fetichism, Religion and Magic: Priest and Sorcerer, the Soul: Immortality and Transmigration, Feeding the Dead and Sacrificial Rites, Religion and Mythology. A second group deals with the forms of religion, as those of Babylon, Egypt, China, India, Greece, Rome, Scandinavia, Mazdeism and Zoroaster: Mithraism, Buddhism, and Mohammedanism. A short, selected bibliography ends the work.

Such a Primer is a difficult book to write, and an extremely easy book to criticise. It demands not only wide and varied learning, but it demands that the writer take up, as occasion requires, the attitude of the philosopher, the psychologist, the historian, and the anthropologist. Needless to say, it demands absolute impartiality: though the publishers of the present work may well have found their account in entrusting its preparation to a clergyman. In the reviewer's opinion, however, the fatal defect of the Primer lies in its author's innocence of any scientific psychology. The naïve assumption of a religious faculty (15, 19); the citation of Myers's psychorragy (71); the ascription of religious doctrine to the 'soul' and of mythology to the 'imagination' (87); the lack of reference to modern psychological studies: such sins of omission and commission cannot but inspire the psychological reader with distrust. It is in no carping spirit that the further criticism is made that sometimes (as in the bracketing of Tylor and Spencer: 14) the writer's facts are at fault. M. W. WISEMAN.

Selections from the Music Dramas of Richard Wagner: arranged for the Piano by O. SINGER. With a preface by R. ALDRICH. Boston: Oliver Ditson Co. pp. xviii, 194. Price (cloth), $2.50.

Twenty-four Negro Melodies: transcribed for the piano by S. COLERIDGE-TAYLOR. With preface by B. T. WASHINGTON. Boston: Oliver Ditson Co. pp. ix, 127. Price (paper), $1.50.

These two books, Nos. 15 and 17 of the well-known "Musicians' Library," are of especial interest to psychologists. Mr. Aldrich's preface gives a biographical sketch of Wagner, and a brief analysis of the operatic situation in the various selections made by Mr. Singer. He reproduces further the programme-note written for the Tristan Prelude and the close of the Prelude itself (this in facsimile), and adds a useful bibliography. The twenty-five selections—all within the compass of a moderately skilled performer—are taken from Rienzi, the Flying Dutchman, the Rhinegold, Siegfried (one each); Tannhäuser (2); Lohengrin, Tristan and Isolde, the Walküre, the Gotterdämmerung, Parsifal (3 each); and the Meistersinger (4). No one of them could well be spared; and if there are a few more whose omission one deplores, it must be remembered that the book had its limits. A reproduction of the last photograph of Wagner taken from life forms the frontispiece of the volume.

Mr. Washington outlines the life and works of Coleridge-Taylor, and offers some general remarks on the qualities of Negro music. Of the twenty-four compositions included in the book, four are based on themes from southeast Africa; two come from south Africa; one each from west Africa and the West Indies; the remaining sixteen are based upon American Negro melodies. The themes and the handling of the themes are alike interesting, æsthetically and psychologically. A portrait of the composer serves as frontispiece. P. E. WINTER.

The Art of the Musician, a Guide to the Intelligent Appreciation of Music, by H. G. HANCHETT. New York: The Macmillan Co. pp. viii, 227. 1905. Price, $1.50 net.

"This book is the outgrowth of a course of popular lectures intended to give to persons fond of music, but not thoroughly versed in its intricacies, an idea of the reasons which prompt musical critics to approve or disapprove of musical compositions. . . . It is designed to emphasize the distinction between the real study of music and the study of the arts of playing and singing which has so long been mistaken for it. . . . It aims to supply such information as should make concert-going more satisfactory, listening to music more intelligent. . . . The ability to understand musical notation is all that is presupposed of knowledge of the art." These, in the writer's own words, are the aims of the present volume. In the reviewer's opinion, they have been successfully attained. In a course of popular lectures we expect to find, here and there, statements that are too sweeping, as well as statements that are unduly definite. The writer's teaching, however, is in general so sound that the few slips of this kind—none of them important—may readily be condoned. The work, on the whole, is excellent.

Ch. i (Art and Music) lays much-needed emphasis on the intellectual aspect of music. "A fine art is the conscious or intentional utterance of thought, by word or action, for the purpose of creating beauty or expressing emotion;" "the art of the musician is the conscious, intelligent or intentional handling or combination of sounds, rhythms, musical conventions, and inspirations, into works displaying beauty or expressing emotion." Definitions that are, truly, only proximate; but sound as far as they go. Admirable, here and elsewhere in the book, is the author's insistence that melody is a *datum*, part of the materials of the musician, not a product of his art. Ch. ii (The Material of Music) treats of tone-color and of dynamic expression. Ch. iii (The Life of Music) discusses time, rhythm and meter; the chapter, which is one of the best, has 26 musical illustrations. Ch. iv (The Soul of Music) deals with harmony, and ch. v (The Beauty of Music) with the organization of melody (7 and 15 illustrations). They are altogether untechnical and unconventional, and contain much good analysis. Ch. vi (The Germ of Music) takes up the motive, including the Wagnerian leading motive (12 illustrations). Then follow chapters on thematic development (15), counterpoint and fugue (3), form-building, classical music (2) and romantic music (1 illustration). To mention in these last chapters are the analysis of the scherzo of Schubert's first sonata, and the remarks on verbal interpretations of romantic music. Ch. xii characterizes the Art of the Interpreter. From ch. xiii (Musical Education) we should like to quote *in extenso*. "What we need is education in *music*. Not more professors, but more amateurs; not more concerts, but more intelligent interest in those we have; not more compositions, but more comprehension; not more vocal culture, but more and larger choral societies; not more technic, but more interpretation." "The crying need . . . is not higher but broader culture, not more musicians but more music lovers, not more technic but more understanding." To these words every psychologist who is musically minded—and can one be a psychologist without the musical mind?—will unreservedly subscribe. Finally, ch. xiv (The Test of Musical Worth) sums up the teaching of the book in the sentences: "The appeal of music is to the emotions, but it is an intelligent appeal. Perhaps its greatest virtue is its power of lifting the mind above sordid cares and worries, and giving pleasure, stimulus, peace, and rest; but that power is multiplied many times by a thorough understanding of the structure and secret of the art."

The 81 illustrations are well chosen, and the book gains greatly, of course, if read with a piano at hand. The reader must, however, resolutely break the back of the binding, if he is to make the volume lie flat upon the rack.

P. E. WINTER.

My Little Book of Prayer, by M. STRODE. Chicago: Open Court Publ. Co., 1905. pp. 77.

This is a little book not so much of prayer, in the accepted sense of that term, as of moral and religious reflections thrown into parable or aphoristic form. It shows the influence not only of the great Christian sources, but also of Marcus Aurelius and—Omar Khayyam. The writer is not quite sure of the distinction between 'shall' and 'will,' and, in general, fails at times to preserve the desired dignity of phrase. It is almost comic to read: "May God forgive you your weakness—but let him damn mine," despite the elevation of the sentiment; and the ejaculation "Dear Heaven, I am a Hercules of disseminated force" is hardly a specimen of English undefiled. M. W. WISEMAN.

The Napoleon Myth, by H. R. EVANS. Containing a Reprint of *The Grand Erratum*, by J. B. PÉRÈS, and an Introduction by P. CARUS. Chicago: the Open Court Publishing Co., 1905. pp. 65.

"The sole excuse for republishing M. Pérès' 'Grand Erratum,'" says Dr. Carus in his introduction, "is the fact that it is out of print and forgotten." The excuse is sufficient—quite apart from the intrinsic interest of the brochure, which Dr. Carus naturally compares and contrasts with Whately's "Historic Doubts." The book is lavishly illustrated with Napoleon-pictures, and makes an attractive and acceptable volume,—except that, in his essay on "The Mythical Napoleon," Mr. Evans has written somewhat scrappily, without any serious effort to make the most of his subject. P. E. WINTER.

Der doppelte Standpunkt in der Psychologie, von MARY WHITON CALKINS. Leipzig, Veit und Co., 1905, pp. 80. To be had in the United States from C. A. Köhler, 149 A Tremont St., Boston.

An Introduction to Psychology by MARY WHITON CALKINS. New York, The Macmillan Co., 2d Edition, 1905.

The "double standpoint," to the exposition of which Professor Calkins devotes the German pamphlet mentioned above, may be indicated by the following brief quotation from the second section of the paper where she takes up her chief problem. "The thesis which I seek to uphold," she says, "is the following: Consciousness can be considered psychologically from two standpoints: It may be conceived either as a series of connected psychical occurrences, with no reference whatever to a conscious Ego, or as a many-sided consciousness of a particular Ego in its relations. Following from these two view-points arise two sorts of psychology: A phenomenal psychology (*Vorgangspsychologie*) and an Ego-psychology (*Ichpsychologie*)" (pp. 32-33). The first of these is the professed ideal of many modern psychologists, however shiftingly they may hold to it at different points in their psychologizing. Professor Calkins contends, on the contrary, that this sort of psychology, capable though it is of perfectly self-consistent carrying out in all respects, is insufficient, because it neglects the fact that all consciousness is personal, "that every consciousness belongs to some sort of Ego." The Ego which is central to the Ego-psychology is too fundamental for exact definition, but the consciousness of it can be described in part, and shows also this important relation, to wit, that it is social; one knows himself only in relation to other selves.

Professor Calkins's argument in the remainder of the paper is directed to showing by an examination of concrete psychical experiences that the phenomenal theory is insufficient by itself for the explanation of the full richness of the mental life.

The same double standpoint was influential with Professor Calkins earlier in the preparation of her well-known text-book. A second edition of this has now permitted a number of minor changes, and made an excellent work a little better still. E. C. S.

Schriften der Gesellschaft für Psychologische Forschung, Heft 15 (III Sammlung). Johann Ambrosius Barth, 1905. p. 583-692.
Psychologische Faktoren des Modernen Zeitgeistes, von RICHARD BAERWALD.
Die Bedeutung des Urteils für die Auffassung, von PAUL MÖLLER.

The first writer discusses the psychological factors of the spirit of modern times. Under this general theme he treats of the possibilities of a historical psychology, the dominance of rhythmic type in German painting, concrete and abstract types, and the feeling for ideas. He believes as compared to the time of Goethe the present is a concrete epoch, but that there is an alternation between these points of view. An interesting chapter is that dealing with mixed feelings which he thinks dominate to-day. These complexes are of the stimulating and not of the restful type. He thinks that the tragic element has in recent times retreated to make room for the naturalistically sad, and that our feelings are now a little dulled.

Ophthalmic Neuro-Myology, by G. C. SAVAGE. Published by the author, Nashville, Tenn., 1905. pp. 221.

This book attempts to make the ocular muscle problem easy. The hypothesis on which the author bases everything is that there are eight conjugate brain centres in the cortex by means of which the several elements of vision are effected, and one conjugate centre controlling convergence. These centres act like orthophoric and heterophoric eyes, and when there is only one eye. Each of these is connected with two muscles, and the work done by the centre and its muscles under guidance of volition is normal. These centres have no causal relation with heterophoric states and cannot correct them. Thus there are twelve basal centres which connect with only one muscle. If the eyes are emmetropic-orthophoric these centres are forever at rest, but if there is any form of heterophoria one or more centres must be forever active when we wake. Centres do not cause the heterophoria, but are ready to correct it, guided by a "fusion faculty." Each basal centre is ready to act on its muscle whenever there is a condition that would cause diplopia.

L'Année Psychologique, publiée par ALFRED BINET. Onzième Année. Masson et Cie., Paris, 1905. pp. 693.

In this number we have a rather unusual number of interesting memoirs, mostly by Binet and his pupils. Those most significant are on mental fatigue, the science of testimony, measure of intelligence, association of ideas, the tactile sensibility in organic hemiplegia, the asymmetry of the gustatory sense, metaphysics of sensation and of the mental image, the method of measuring the degree of instruction, etc. These take up the first 336 pages. Then follow general reviews, extending to the 572nd page. The remainder of the book is devoted to an analysis of the more important literature. It is greatly to be deplored that this part has now shrunk to a trifle over a hundred pages. It is no disparagement to the papers of Binet and his friends to say that it is the bibliography to which chief importance is attached, and the diminution of which is sure to be generally deplored.

Das Ich und die sittlichen Ideen im Leben der Völker, von O. FLUGEL. Vierte Auflage. Hermann Beyer & Söhne, Langensalza, 1904. pp. 270.

We here have a revision of the fourth edition of a work first published in 1889 which treats the problem of the ego in a comprehensive and somewhat novel way. Stress is laid upon the fact that of all philosophic problems this was the last to appear and to become prominent. The ego is first treated in its relations with the body, then to the environment. An interesting chapter shows how the name of a person is the ego itself heard, and how relationship is often expressed by similarity of names. It is then treated as the personifying element in the apprehension of nature; then as an active principle expressed in self-feeling, irritability, dance, penance, the projection outward; and lastly, the abstract ego, its kernel, the migration of souls, absorption in Nirvana, and the individual and social self. The next and longest chapter treats of the development of the moral ideas, especially those of beneficence, perfection, right, justice, inner freedom, and the influence of religion upon morals, and last of all, the absolute in ethics.

The Night Side of Nature, or Ghosts and Ghost Seers, by CATHERINE CROWE. E. P. Dutton & Co., New York, 1904. pp. 502.

Here we have an old book reprinted as new, full of ghost stories told in the most real, matter-of-fact way by a profound believer in them. The evidence is often given in great detail, and the marvels, if we accept the text, are often simply stupendous. We wonder why the members of the Psychic Research Society have overlooked the work, which so abounds with evidences of telepathy and spirit agency.

Der Befruchtungsvorgang: Sein Wesen und seine Bedeutung. Von ERNST TEICHMANN. (Aus Natur und Geisteswelt. Sammlung wissenschaftlich-gemeinverständlicher Darstellungen.) B. G. Teubner, Leipzig, 1905. pp. 102.

This commendable primer opens with a history of the problem of fertilization, and then discusses successively cell division, germ cells, fertilization, ripening of the germ, the division and conjugation, the chromosomes and mixtures of qualities, and ends with a brief résumé of the literature.

Biographic Clinics, Volume 3. Essays Concerning the Influence of the Visual Function Pathologic and Physiologic upon the Health of Patients. By GEORGE M. GOULD. P. Blakiston's Son & Co., Philadelphia, 1905. pp. 516.

In this third volume Dr. Gould continues his studies upon the effect of eye troubles upon general health. Here he discusses, as salient instances of life-long suffering that might have been avoided, John Addington Symonds and Taine. Dr. Snell discusses eye strain as a cause of headache and other neuroses, and Dr. C. E. Pronger describes the influences of slight errors of refraction on the nervous system. In the sixth chapter the author resumes the work and discusses the history and etiology of megrim, the ocular factors in scoliosis and their bearings upon handwriting, dextrality, and sinistrality, with their pathological results. Subsequent chapters are on subnormal accommodation and premature presbyopia, on the reception of mental discoveries, post-mydriatic refraction tests, and a mathematically perfect eye. The author advocates pens with angle holders which permit an unobstructed view of the nib of the pen as it moves, without malposture of body, hand, paper or head.

The Psychic Treatment of Nervous Disorders. PAUL DUBOIS. (Translated by Smith Ely Jelliffe and William A. White.) Funk and Wagnalls Co., New York and London, 1905. pp. 466.

This is a translation of Dubois's Les Psychonevroses et leur Traitment Moral, which became so popular in the original as to necessitate a second edition within a short space of time. To a stranger or novice in psychotherapy, this work would seem to be an emanation of Christian Science, except that it is pre-eminently more scientific. The author is certainly an enthusiast along his particular line of psychic treatment and for him, sound moral persuasion is the universal panacea for all the functional nervous ills that flesh is heir to. His unbounded enthusiasm and self-confidence have led, however, to a certain amount of figurative blindness on other sound therapeutic measures, even to waking suggestion and hypnosis, unless, indeed, he includes the former in the category of moral treatment. Starting with a few psychological considerations, at times lacking so much in clearness as to become almost metaphysical, he discusses the manifold symptomatology of various functional nervous conditions, but always with a commentary strongly colored by his own individuality. In the same vein and with the citation of many cases, he discusses hysteria, mild periodic depressions, various phobias and fixed ideas, gastro-intestinal, circulatory, cardiac, and urinary disorders. He insists on isolation, rest in bed and over-feeding as an important adjunct to his moral conversations. There is much moral and ethical preaching, much repetition and many of his points lack force, not because of their sound basis and intrinsic merit, but because of much redundancy of language and a very conscious striving after rhetorical effect. The translation is well and faithfully done, with the exception of a very obvious fault of the English rendering of the title. I. H. CORIAT.

Investigations of the Departments of Psychology and Education of the University of Colorado. Vol. II, No. 3, June, 1905. University of Colorado, Boulder, Colo. pp. 37.

This number is perhaps on the whole the best of this series. Its articles are on the home study of geography, history, physics, economics, mathematics, and modern language in the high school, the teaching of botany and zoölogy. All are interesting and some are quite suggestive articles.

Psychologie und Pathologie der Vorstellung. Beiträge zur Grundlegung der Æsthetik, von RICHARD WALLASCHEK. Johann Ambrosius Barth, Leipzig, 1905. pp. 323.

The first part discusses psychic expression and its diseases in language, song, reading, writing, gesture, action, and the explanation of these. The second part is devoted to inner psychic life and the essence and lapse of concepts, treating of their composition, association, memory, the natural and artificial diseases of concepts, and natural and artificial sleep.

The Foundations of Philosophy. A Study of the Doctrine of Degrees and Related Subjects, by GEORGE TROBRIDGE. James Spiers, London, 1904. pp. 94.

Besides the doctrine of degrees the author discusses the philosophical value of the doctrine and its practical bearings. His interpretation of the doctrine is essentially ethical and religious. He even makes it include pedagogy.

Proceedings of the Society for Psychical Research. Part L, Vol. XIX, April, 1905. Brimley, Johnson & Ince, Ltd., London, 1905. pp. 77.

Weitere Untersuchungen über Beziehungen zwischen geistiger Ermüdung und Hautsensibilität, von H. GRIESBACH. Sonderabdruck aus Internationales Archiv für Schulhygiene, I. Bd., 3. Heft, pp. 317-417. Wilhelm Engelmann, Leipzig, 1905.

Physiological Psychology, by W. McDougall. (Temple Psychological Primers.) J. M. Dent & Co., London, 1905. pp. 172.

Beiträge zur Psychologie der Aussage, von L. William Stern. Zweite Folge. Zweites Heft. Johann Ambrosius Barth, Leipzig, 1905. pp. 154.

The Response of Inorganic Matter to Stimulus, being the Friday Evening Discourse at the Royal Institution, May 10, 1901, by Jagadis Chunder Bose. Wm. Clowes & Sons, Ltd., London, 1901. pp. 24.

NOTES AND NEWS.

THE ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION.

The fourteenth annual meeting of the American Psychological Association was held Dec. 27, 28, and 29th at Emerson Hall, the new Harvard building devoted to philosophy and psychology. The opening session was at nine o'clock, Wednesday morning, the president of the association, Miss Mary Whiton Calkins, Professor of Philosophy and Psychology at Wellesley College, presiding. The first paper was by G. V. N. Dearborn, of Tufts Medical School, on the Relations of Muscular Activity to the Mental Process, followed by a paper by Irving King, entitled "How can the Relation of the Conscious to the Subconscious be best Conceived?" Mr. King's point of view being that consciousness is best conceived as a point rather than a field capable of being graded off into the subconscious. These first two papers were included in one discussion. The next group of four papers was devoted to Comparative Psychology. The first, by Dr. Yerkes, of Harvard, on the Senses and Intelligence of the Japanese Dancing Mouse, was rendered especially interesting by the exhibition of the mice themselves, who showed the peculiar movements characterized as dancing. This study is still in progress but so far as can be stated from present results, the animals seem to be degenerate and below the standard of common mice in sense and muscular development. Mr. Porter, of Clark University, then gave a five minute report on the Psychology of the English Sparrow, followed by a longer paper on the Habits and Instincts of Spiders, genera Epeira and Argiope. Mr. Porter's extensive observations show for these two species a variability in instinct and a distinct adaptation to environment in the web spinning. Mr. Davis's paper discussed a different species of spider, which does not spin webs but forms its nests by the binding of grass blades. The character of these nests is such that in their variability Dr. Davis thinks we may have a possible objective measurement of the variability of instinct. Prof. Wheeler's paper on the Ant Queen as a Psychological Study brought out some exceedingly interesting points in the life history of the ant queen, which was shown, in many respects, to be quite the opposite of that of the queen bee, since the ant queen unites in herself all the characteristics and energies of the worker as well as the reproductive functions. The next paper, by Dr. Edward Cowles, was a Study of the Physiology of Conscious Experiences, and was a plea for function rather than structure as a correlate of pathological mental conditions. This was followed by a paper by Dr. Boris Sidis on the Nature of Hypnotic and Post-Hypnotic Hallucinations. Dr. Sidis is convinced, from his experiments in hypnosis, that there are no true hallucinations in either hypnotic or post-hypnotic states but illusions which the subject really knows to be such.

The morning session closed with a paper on the Psychology of Sudden Conversions, by Dr. Morton Prince, who has made a detailed study of a single case, in which he was able to recover by hypnosis the mental experiences of a trance state, of which the subject retained no memory in a waking state. The emotional experiences of conversion following this state, Dr. Prince ascribed to a strong revival of previously experienced religious emotions during the trance, which was carried over into the conscious state without memory of their cause.

At the conclusion of Dr. Prince's paper the association adjourned to the Harvard Union as guests at a luncheon given by the Harvard Corporation.

At 2.30 P. M. there was a joint meeting of the American Psychological Association and the American Philosophical Association and the first part of the session was devoted to the exercises of the formal opening of Emerson Hall. The opening address was made by Pres. Eliot and this was followed by the principal address on Ralph Waldo Emerson, by Dr. Edward Emerson. The dedication exercises were followed by a joint discussion before the two associations on the Affiliation of Psychology and Philosophy and with the Natural Sciences. The discussion was opened by Prof. Münsterberg, who expressed himself strongly in favor of the close affiliation of psychology and philosophy and a withdrawal from the natural sciences. He was followed by Pres. G. Stanley Hall, of Clark University, who took the opposite view and would withdraw psychology from philosophy and affiliate it with the natural sciences, since, from his point of view, psychology, provisionally defined, is "a description as accurate as may be of all those facts of psychic life, conscious and unconscious, animal and human, normal and morbid, embryonic and mature which are demonstrable and certain to be accepted by every intelligent unbiased mind which fully knows them. They must also be so ordered like to like, and organized, that they can all be known with the least trouble. The best plan of organization when possible is evolutionary. Under this definition, psychology is excluded from no field of experience, conscious or unconscious, religious, social, genetic or individual that can be studied on the basis of solid empirical data and hence its closest allies as an inductive science in the future must be biology, physiology and anthropology. The nature of soul no more concerns it than does the ultimate nature of matter and motion concern physics. Such discussions belong to philosophy, the history of which Dr. Hall would insist upon as a part of the training of every experimentalist but would avoid too prolonged a lingering in philosophical fields, lest it unfit for dealing with facts. Psychology, he thinks, is yet in its dawn and its striving should be toward the goal of becoming a true natural history of the soul. The discussion was continued by Profs. Thilly, Angell, Taylor and Witmer, the points of view lying between those of the first two speakers.

At 7.45 in the evening the association met to listen to the address of the President, on A Reconciliation of Structural and Functional Psychology. After a very clear statement of the methods of structural and functional psychology, whose relation was compared to that of histology and physiology in the natural sciences, President Calkins proposed psychology-considered-a-science-of-the-related-self as a mediation by which the methods of both structural and functional psychology could be utilized and reconciled. The President's address was followed by a reception at the home of Prof. and Mrs. Münsterberg.

The morning session on Thursday was opened by two papers on the definition of feeling; the first by Mr. Henry Rutgers Marshall, the second by Prof. Norman Gardiner, of Smith College. These two papers summed up the various views of feeling and paved the way for a general discussion, in which Professors Angell, Duncan, G. Stanley Hall, Judd and Royce took part. The present ambiguities and difficulties of the word were fully discussed but no satisfactory substitute was suggested. Dr. Hall proposed to cut the Gordian knot by dispensing with a formulated definition until we had more knowledge on the subject, since, as he believed, we were just now in need of facts more than of a definition. As a working definition, the one proposed by Miss Washburn was perhaps the one which could be most generally accepted. Excluding pain and adopting unpleasantness as the opposite of pleasantness, she would define feeling as an unlocalizable and unanalyzed mental state. The discussion, while not furnishing any unified definition of feeling, brought out with great clearness the necessity of careful limitations in the use of a term so ambiguous.

The next two papers were on Attention—the first by Dr. Burnham, of Clark University, on Interest and Attention, in which interest and attention were regarded as identical and as intense states of consciousness, present as aspects of growth. The second paper, by Dr. Hylan, of Harvard, was a careful and detailed experimental study of attention and its limitations. The next paper, on the Psychology of Organic Movements, by Dr. I. Madison Bentley, of Cornell, was a plea for a more psychological study of organic movements, in distinction from the numerous biological and psycho-physical methods of most of the studies in this line. This was followed by Prof. Stratton's discussion of Modified Causation for Psychology (a modification of Maine de Biran's theory), after which the society adjourned to the hall of the philosophers to listen to the address of Prof. Wilhelm Ostwald, of Leipzig, on Psychical Energy. Prof. Ostwald's thesis was that as energy may be made to explain all physical phenomena, so likewise it may explain all psychic facts. Energy is found everywhere in the world; it is the most general property whose essence or test is work. Wherever work of any sort is accomplished, there we have energy. In eating, the chemical energy of food is transformed into psychic energy. When this store of energy has been used up more must be added in some way before work can go on. Prof. Ostwald thus regarded all psychology as reducible to energetics. This view would play havoc only with dualistic systems based on the old mechanical views of matter. In the discussion which followed Prof. Royce brought up the objection that physical energy, as we know it, is definitely measurable and he did not see how this concept could be applied to all the facts of mental life. Some activities were measurable but to others the principle of energetics seemed inapplicable. Prof. Ladd also asked some questions in regard to the specific application of the principle, and the session closed with a hearty expression of thanks by Dr. Hall to Prof. Ostwald for his suggestive and invigorating address, whose point of view he regarded as most helpful for modern psychology and irreconcilable only with the old-fashioned double housekeeping philosophy which he regarded as the resort of a mind not yet in order.

The afternoon session opened with a discussion (1) of the possibilities of co-operation between laboratories and departments of different universities and (2) on Elementary Instruction in Psychology. The opening paper was read by Prof. Judd, who said that he regarded co-operation as regards courses of instruction impracticable since individual interests must largly determine the line of work. He suggested, however, that since the larger laboratories often manufactured some pieces of apparatus, it would be helpful if there was some centre where information in regard to the pieces manufactured by each laboratory could be obtained. It was also suggested that an interchange of the studies made in each laboratory would be a mutual advantage. Prof. Sanford then outlined a Beginner's Course in Psychology which the President aptly characterized as "invigorating heresy." Prof. Sanford said that in teaching psychology, two questions were possible, namely: how can the subject be presented in the best possible way to give an idea of the science itself, and how can it be presented to best fit the actual needs of the class in the relations of life? The second question was the one which Prof. Sanford had asked himself and his answer was the following course:

1. Psychology of Learning and Acquisition, including memory, habit, practice, acquirement of manual skill and dexterity, learning of language and complex mental operations.
2. Psychology of Truth and Error, including comprehension, belief, illusions, prejudices, superstitions, delusions, psychology of testimony, etc.
3. The Nature and Laws of Emotion, including the strong racial emotions, their genesis and hygiene and the psychological basis of Æsthetics and Ethics.
4. Psychology of Character and Personality, including types of character, criminal, pauper, mystic, philistine, psychology of leadership, etc.
5. Facts of the interdependence of mind and body: the permanent and alterable in human character, heredity and acquired character, hypnotism, mental disease and hygiene.
6. Psychogenesis: biological view of mind and its history in the animal series; human psychogenesis.
7. Systematic Psychology: a short review of the definitions and laws to be found in some small text-book.

At the close of Prof. Sanford's paper the subject was opened to general discussion. It was suggested that the course would take a lifetime for completion, but this would also be true of any good cause in psychology, which gives only an introduction to the subject. Miss Gamble, of Wellesley, brought up the fact that with large classes and limited library facilities there were practical difficulties in carrying out such a scheme.

The evening session was devoted to the address of the President of the Philosophical Association, Prof. Dewey, whose subject was Beliefs and Realities. Prof. Dewey's address was followed by a Smoker, while the ladies of the two associations were entertained at the home of Mrs. Royce.

The first paper of the Friday morning session was read by title, as the author, Mrs. Christine Ladd Franklin, was not present. Mrs. Franklin's subject was the Doctrine of Specific Energies. The remaining papers of the session were devoted to various phases of the psychology of vision, the first group of three papers, the Color Sense of Young Children, by W. S. Monroe; Primitive Color Names and Primary Colors, by J. W. Baird; and a Study of After Images on the Peripheral Retina, by Kate Gordon, dealing with problems in color vision. Prof. Monroe's paper was a report of tests made on young school children. The thesis of the second paper was that, from the evidence derived from philology we can infer color sensitivity in primitive peoples. Prof. Gordon reported the results of an experimental study made with great care and accuracy in the Mt. Holyoke psychological laboratory.

The next paper, Visual Adaptation in Tachistoscopic Experimentation, by J. A. Bergström, was read by title. The remaining four papers: Photographic Studies of Convergence, by C. H. Judd; The Relation of Eye Movements to Judgments of Number, by Robert MacDougall; Vision during Dizziness, by E. B. Holt; and Vision and Localization during Eye Movements, by R. S. Woodworth, were grouped and included in one general discussion which was carried over to the beginning of the afternoon session. The main point about which the discussion centered was whether visual consciousness exists during rapid eye movements. Dr. Holt, from his experiments, inferred complete anæsthesia, but later was inclined to accept Dr. Dodge's term, inhibition, as covering the facts. Prof. Judd and Woodworth contended for visual consciousness during eye movements. The last paper of the morning session on the Possibility of Retinal Local Signs of the Third Dimension, by W. P. Montague, was read by title, as was also the first paper of the afternoon programme,—A Simple Method of Measuring Relationships, by E. L. Thorndike. Prof. Kirkpatrick's paper on the Growth of Vocabularies was a preliminary report on proposed means of measurement. Dr. Seashore, of Iowa University, then gave a description of the Voice Tonoscape, an instrument which he has perfected for the training of the finest voices and of which experimental tests appear to demonstrate the value. The next paper was by Frederick Lyman Wells on Linguistic Lapses, followed by a Comparison between the Speed of Imagined and Actual Rhythmic Muscular Movement, by Charles T. Bennett. This was an individual study and brought out the surprising fact that the imagined movement is slower than the actual movement, Dr. Hylan then gave a short demonstration of a new Kymograph, and the Association adjourned to the new laboratories where an informal conversazione of experimentalists was held.

Additional papers on the programme read by title were as follows: An Experimental Study in the Psychology of Voting, Colin A. Scott; Sex Differentiation in the Sense of Time, Robert MacDougall; Some Psychological Aspects of Success, Brother Chrysostom; Early American Psychology, I. Woodbridge Riley. THEODATE L. SMITH.

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CRYING.

By ALVIN BORGQUIST, Research Assistant, Clark University.

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I. INTRODUCTION.

Statement of the problem. Crying, far more than its opposite, laughing, is a neglected problem of psychology. This peculiar complex of characteristic movement and feeling, which has such a prominent place in the emotional life at all ages, is very rich in its suggestiveness as a psychological problem. Besides the light which an explanation of crying may throw upon a general theory of the emotions, there arise special questions in regard to its development and to the nature of the particular associations which it involves between the mental state and the physical expression.

Standing as it does at an end of the emotional scale as the expression of extreme displeasure, its sharp contrast with laughing—expressive of the higher states of pleasure—is significant, especially when the extreme psychological similarity of the two phenomena is taken into account. The question may be raised at the outset whether we are not here dealing with fundamental modes of expression which are the basis of pleasure and displeasure in their most general form. The cry, as expression, is closely related to language development—a phase of the subject in which ethnological questions cannot be avoided.

In its extreme forms, crying presents problems which, though properly coming within the province of the alienist are also psychological, and which, when solved, will be helpful in the interpretation of normal emotion. The hygienic aspect of the crying state is important both in itself and for conclusions of more general pedagogical application that may come from its discussion.

Sources of material. The data upon which the present study of crying is based consist in part of 200 returns (161 females and 39 males) from a special questionnaire sent out by President G. Stanley Hall. The questionnaire is as follows :

1. As a child did you ever cry till you almost lost consciousness or things seemed to change about you? Describe a cry with utter abandon. Did it bring a sense of utter despair? Describe as fully as you can such an experience in yourself, your subjective feelings, how it grew, what caused and increased it, its physical symptoms, and all its after effects. What is wanted is a picture of a genuine and unforced fit or crisis of pure misery.

2. Describe such a fit of crying, either of an individual case or a composite photograph of it in other children you have seen.

3. Do you now have crying spells, with inadequate cause, at certain seasons, when you give way to tears and sobbing when alone or with others? If so, describe a typical case or two. Did you feel great easement, rest, refreshment, or relief afterward, and is occasional crying a good thing and too much repression bad?

4. Describe each symptom of a "good cry" in order. (1) Are tears first and can they be repressed? Have some a weakness for lachrymose symptoms, as for blushing? Is it sometimes a mere secretion of overloaded glands, with little psychic pain, taste of tears, and is it always the same? Do tears cease early in the spell, differ in quantity, are they more in one eye than the other, is there a difference of age, sex, personality in this? (2) Describe lump in the throat and its repression, effects on respiration, on the voice, complexion and circulation.

Is there nausea? (3) Describe vocalization of the cry in old and young, individual cases. What is a sob? Describe its frequency and culmination. Is there physical pain and where; in throat, thorax, abdominal walls? Where is the seat of convulsiveness? Is sobbing the climax of the cry? (4) Where and what are the first signs of alleviation? Describe stages of recovery, symptoms of a recent cry, psychic retrospection. Are there stages or symptoms you specially like or dread?

5. Are there cry fetiches, *i. e.*, special acts, thoughts, experiences or scenes that have pre-eminent power to cause it in you? What conditions, internal and external, are most and least favorable?

6. The artificialities of crying. When and how are you impelled to cry for effect or intensifying symptoms? How far can control of crying be itself controlled by will? How does resentment modify the genuine expression of pure grief or psychalgia? Is there desire to excite pity? What are the effects of fear, or shame? Having felt the impulse to cry, can it be so entirely repressed that later, when alone, you are more impelled to vent it and yield? What symptoms can be most and what least controlled, or controlled for effect? Do you cry *to* people? Describe crying as a source of pleasure in power to control the feelings of others, to draw their sympathy, or cause them to regret their acts. Describe effects of the tears of parents, teachers and friends on our conduct.

7. Describe an angry cry, its phenomena, occasions, results. Describe effects of yielding to children's tears on them. Describe the child spoiled in this respect.

8. How are your crying habits modified by age as to frequency, cause, duration, provocation, symptoms, and reactions afterwards?

9. In what respect is crying good: in infants, children, adolescents, in maturity, and in what bad? Should it have a place and a cult?

10. Can you describe cases of crying in persons usually self controlled when the final break down comes as a cumulative effect? Does such crying tend to become hysterical? Is it followed by physical prostration? Is it a cause or an effect of physical weakness?

Returns were received from the State Normal School, New Paltz, New York; State Normal School, Westfield, Mass.; School of Ethical Culture, New York City; State Normal School, Emporia, Kansas.

This material has been extensively supplemented by ethnological data secured, for the most part, through the Bureau of American Ethnology, and its reports and from the Archives of Aboriginal Knowledge. Twenty special letters and questionnaires were also sent out, some to the Ethnologists of the Smithsonian Institution, under the direction of W. H. Holmes, Chief of the Ethnological Bureau. In correspondence with missionaries, other material was obtained relating to the inhabitants of various islands of the Pacific, the Japanese, Samoans, New Zealanders and Maoris.

A copy of a letter sent to a missionary among the Maoris will indicate the nature of the ethnological material obtained:

(1) Do the Maori cry? (2) If so, what are the elements composing the phenomenon so far as they are apparent—tears, sobs, vocalization, changes of complexion, attitudes, movements, contortions of the face,

etc., if any? (3) What are the leading occasions or provocations—fear, shame, anger, pain, grief, etc.? (4) Intensity, duration, after-effects, physical and mental; *e.g.*, does he remain depressed or soon recover and become hilarious or cheerful? (5) Difference in age or sex?

The subject as it is to be presented is divided into three parts.—Part I contains data and partial interpretation of more general aspects of the subject, the classification of crying states, crying among primitive peoples, influence of age, cumulative development, physical causes and symptoms, and description of the mental states in crying.

Part II makes an analysis of the crying act as a series of physical events, such as circulatory symptoms, attitudes of the body, vocalization, lump in the throat, sob, tears, and the physical effects.

Part III is a general résumé of the present theories of crying, physiological and biological data, development of theory, and general summary of the subject.

GENERAL ASPECTS OF CRYING.

Classification of Crying States. Whatever further analysis may show to be the common or essential elements in the crying state, a cursory survey of the emotional field, over which it extends, reveals the fact that we are dealing with a phenomenon that is widespread and probably exceedingly complex.

In answer to the question what are the special acts, thoughts, experiences or scenes, internal or external conditions that have a pre-eminent power to cause crying, 307 occasions were mentioned. These causes and situations fall into forty-seven groups.

In these groups, all the emotions that are mentioned in Ribot's classification are represented, as are also all of the occasions of emotions noted in President G. Stanley Hall's studies of fear, anger and pity, and even the depressive reactions to darkness.

Doubtless such refinement of classification of causes of crying could be extended still further. Burton in his *Anatomy of Melancholy* finds over seventy different causes of mental depression.

The range of feelings, therefore, over which the crying state extends is so great that it is an open question whether there is any limit to this act within the field of the emotions.

The returns indicate three types of crying situation sufficiently marked for preliminary classification. Besides the typical cry of grief or sadness, two other main forms stand out clearly: (I) A more or less deliberate, largely vocal, cry, best represented in the crying of anger and less perfectly by the crying in fear and in pain: (II) The cry in joy, including such forms as cries of gratitude, of tender emotion, of feelings of admiration and for the sublime.

Between these type forms all conceivable mixtures and shades occur. In the first group, typical forms of the grief are occasioned by homesickness, remorse, bereavement and melancholy or the "blues." The following cases from the returns will serve as illustrative examples of this class:

F., 22. Grief at sale of the old homestead; "I wept for hours until too weak to care for meals or companions.—Then I dropped into a troubled sleep and for days I was ill."

F., 20. "When ten the teacher cried when reproving me, which brought on an overwhelming fit of crying on my part."

F., 22, "blues." "Often when I am alone I feel, all of a sudden, as if I were alone in the world. Such an awful sense of homesickness comes over me at such times that I usually cry until the fountain of tears seems to dry up."

F.— Grief at death of brother. Long spells of crying; "felt utterly forsaken and alone, and desired to have no one near me."

These will serve to show the grief types, and the characteristic phraseology and tone of the replies.

The following illustrate the more sthenic cry, typical of pain, anger and fear.

F. Child with toothache. Tears were profuse; screamed, violent motion, loss of self control.

Anger: F., 19. "The cry did not make me feel better, only more angry."

M., 22. "When six or seven punished unjustly; howled, pulled my hair, wished to die to punish my parents."

Joy and Pleasure: F., 20. Returning home after two years; cried from joy. The spell of crying was of short duration and was not depressing in its effects.

F., — An unexpected pleasure trip caused the tears to flow.

F., Æsthetic emotion. The sea seen for the first time brought tears.

Though the above described classes seem typical, there are many intermediate forms. It will be necessary to examine more minutely the crying state with reference to discovering whether if complex situations are analyzed and allowance made for accidental causes, such as the spreading of nervous energy, all the crying states may not be found to contain a common element, or at least may not be reduced to a few fundamental situations.

Crying among Primitive Peoples. Weeping appears to be a universal characteristic. Even the Indian brave, who according to popular tradition never weeps, sheds tears freely on occasion of the loss of a member of his family. His stoical attitude is limited to physical pain and his relation to his enemy.

Grinnel says:

"Indians at home and when acting naturally, freely express their emotions. They laugh and chatter and make jokes and cry, shedding actual tears, sometimes with the appearance of great grief, anger or self pity: sometimes with no apparent reason. . . . Mourning for the dead is usually accompanied by the shedding of tears."

Mr. James Mooney writes :

"Indian children cry as long and loud as other children on occasion. They probably do not cry as often, as they are under less restraint both as to discipline and clothing.

"The Indian man seldom cries except under stress of great emotion or on certain ceremonial mourning occasions, but I have frequently seen unrestrained weeping of men on occasion of the death of a child—in one case a perfect outburst of uncontrollable grief of an old man on his daughter's death ; and I have several times seen a man weep while embracing a long absent friend after meeting ; also I have twice seen a father break down while pleading for a son in trouble. A very slight reminder of bereavement by death often starts a woman to weeping aloud, lamenting long after the actual loss. I have known this to happen from the sight of a picture, the hearing of a favorite song of the dead person, or the meeting of a close friend of the lost one. All this is spontaneous.

"There is also the regular death wail, which is more or less ceremonial, except to the nearest relatives, and which occurs immediately after a death, and at intervals for a long period thereafter. With the Kiowas and other Plains tribes every near neighbor, man or woman, tries to join in the first funeral lament, even to the extent of forced tears. The regular death wail is a long-drawn heart-breaking cry, repeated over and over with a loud voice, gradually sinking into silence, the wildest and most pitiful cry imaginable.

"I believe the Indian in general is less excitable than the civilized white man or less impressionable, but gives way readily to grief of bereavement. I have even known a father to starve himself upon his child's grave. My observations relate chiefly to the Plains and the Cherokees."

The historical and legendary accounts of crying are very numerous. The following are examples : "Tears of the mother god figure in the folk lore of many lands. The vervian or verbena was known as the tears of Isis as well as the tears of Juno—a name given also to an East Indian grass (*cox lacrima*)." (4a, p. 119).

"Oriental legend relates that, in his utter loneliness after the expulsion from Paradise, Adam shed such an abundance of tears that all beasts and birds satisfied their thirst therewith." (4a, p. 120). In the Iliad we read of Achilles "To the shore of the old sea he betook himself alone and cast forth upon the purple sea his wet eyes." (Bk. I : 352.) Andromache wept forth her affection. (Bk. VI: 440.) Numerous references to weeping are given in the Bible. "Jesus wept" at the grave of Lazarus. (John 11: 35.) King David wept over the death of Absalom and lamented. "O, my son, would I had died for thee." (II Sam. 18: 33.) "Abraham came to mourn for Sara and to weep for her" at her death. (Gen. 23: 2.) As Naomi urged her daughter-in-law to return home to Moab while she continued to Canaan, "they lifted up their voice and wept." (Ruth 1: 9.)

Crying caused by anger seems also to be widespread among primitive peoples. Historical references to it are frequent. Among the Samoans, the causes of crying that are mentioned are grief, pain, anger and fear. "The Samoan woman cries principally from anger." A reference to the Maoris says, "I have heard women cry when their husbands have beaten them and when they have quarrelled very excitedly." Of the Haida and the Tlinget brave J. R. Swanton writes:

> "When a man has suffered a loss through purely natural agencies or through agencies that he supposes to be supernatural, he often seems to be thrown into utter confusion and gives way to the most childish fit of weeping. Not infrequently, however, he will follow this up with an act of vengeance against the animal or object . . . which he believes to contain an anthropomorphic being which has been injuring him."

Weeping for joy and because of gratitude, admiration, and the tender emotion seems to be mentioned quite as frequently in literature as the cry from grief or anger. "In the Kalevala we read how after the wonderful harping of Wainanpinen, the great Finnish hero, which enchanted beasts, birds and even fishes was over, the musician shed tears of gratitude and these trickling down his body and through his many garments were transmuted into pearls of the sea." (4a, p. 119.) Upon meeting Benjamin, Joseph said "God be gracious unto thee, my son, and he sought where to weep." (Genesis 43: 29-31.) Crying for joy seems to be very common among the negroes. Dr. W. E. B. DuBois writes: "In the religious meetings it takes the place of the old-time shouting." "The Maori woman cries upon meeting her friend." The Sandwich Islanders "cry when they meet their friends as well as when they part."

Some scattered references to racial differences in weeping indicate that the racial variations are partly due to custom and, in part, to other causes: but upon this point there is not enough evidence to show anything conclusively. Among civilized races there are wide differences. Darwin says that the English shed tears much less freely than the people on the continent. Teachers who have Italian children in their classes report that they cry easily; similarly, Mr. Wesley R. Long speaks from a wide acquaintance with the literature of the Latin races, of the abundant references there to weeping and tears.

Influence of age. In 108 of the 126 answers relating to the frequency of the cry in old and young, it is said to diminish with age. The following excerpts and epitomes from the returns will illustrate this point.

Adult. "Have not cried for a long time, eight or ten years."

F., 18. "Have seldom cried since I was thirteen, except when I was sick or nervous."

F., 19. Seldom since six years old.
F., 27. Has crying spells no longer.

Among the eighteen possible exceptions to this statement in regard to the decrease of crying with age, seven refer to the period of adolescence.

F., 21. A chronic crier, but crying was more frequent between twelve and fourteen.

F., 26. Cried most at about thirteen, when growing fast and was very nervous.

F., 18. The worst crying fit was at fifteen. "I seldom cry now, but did two years ago."

Five remark that crying is most frequent at the extremes of life. There are some exceptional cases, for example, a lady, who cries less now, at thirty years, than at twenty-six, but more than earlier in life, though the crying now is less violent. Another at twenty-eight says that crying increases as she grows older, but that the spells do not last so long. All of the returns indicate that crying diminishes with age, with no specially marked rhythms except possibly at adolescence, when there is an increased tendency to crying. A closer investigation of these points might bring out finer and more significant age differences. Differences in the character of the cry with age is probably of more importance.

There is much evidence pointing to significant differences in the character and the causes of the cry as age advances. The motives of sympathy, remorse, bereavement, and pity are apparently largely absent in the young child, but increasingly frequent in later life.

The causes of the earlier cries are largely ego-centric, and sensuous in nature. "The child often cries from pain, and for sympathy;" in maturity the leading motives are grief and trouble, and the infant cries because of bodily need and especially from anger—which is essentially the child cry." "The anger cry comes earlier, than the grief cry." Fear is a leading motive in the child cry. "The cry of the infant or child is largely for the purpose of obtaining something." "The cry of the adult is a cry of grief or of sympathy." These conclusions are repeated in many returns, and expressed in various ways, but leading unmistakably to the conviction that there is a radical change in the character of the cry as age advances. Only eight of the anger cries out of a total of 122 are experiences of adults or adolescents. The cries from fear all belong to the period of childhood, and it is everywhere understood that the cry from pain is tabooed in savagery and civilization alike. The monographs on child study are also in agreement with our returns, while Perez specifically notes that "sympathy is a later development," and cites examples of children in whom it is wanting at four and five years of age (32, p. 79).

Some special evidence bearing upon the age development in regard to the injured social feelings as a cry motive is offered by the work of Lillian A. Russling (unpublished manuscript, on which there is a note in the *Ped. Sem.*, Dec., 1905, 12 : 525), on the attitude, between eight and thirteen years, towards clothing. She shows that the majority of cases, in instances of loss or mutilation of clothing, are expresions of sorrow rather than of anger; that shame is the most prominent element in the complex state of depression here represented and that it increases distinctly with age.

Many reasons are assigned for the decreasing frequency and changed character of the cry with age, a state of affairs that possibly needs more explanation than appears on the surface. The following points seem to stand out. The feelings and desires which have been baffled change or vanish. As age increases the discovery is made that the new desires cannot attain their satisfaction by the early method. Society disapproves of the cry; the desire for approval leads to the abandonment of crying, which as a method of obtaining results comes to be ineffective and is, at the same time, a confession of impotency not in keeping with growing self-respect as the following excerpts indicate.

M., 25. "I realized as I became older that crying was useless."
—"The older I become the less I cry or care to. No doubt this is due to the recognition of the foolishness of crying. You begin to realize that the world cannot be obtained by crying."

Twenty-three replies have to do with the effect of shame in limiting the cry. Shame at being seen crying comes in to arrest its course and to fortify the effort at self-control. Seclusion is more often sought, while distinctions begin to be formed between shameful and legitimate cries; physical pain must be born without crying, while the sympathetic expression of grief is undiminished. One says "I cry less because I am accustomed to disappointment." The discipline of society brings about an increased power of control. The cry advances from the instinctive form to the form of social control that characterizes all emotional development. Thirty-two speak of the raised threshold of crying as age advances; remarking that the cause must be "more serious," "more adequate," "more real." Six state that in their own cases the result is an achievement not of lessened desire or feeling, but due to increased power of control and mastery over the life of feeling. The development of verbal language as an alternative method of expression helps also to diminish the cry. The generic form of demand or complaint that is expressed in the cry breaks up into special expression, linguistic and other. A growing acquaintance with the order in the world sets a limit to desire and expectation ; there is power to forecast the remoter significance of the situation ; the intelligence comes in to devise means of attaining ends.

Among the Indians it appears that the cry from pain is inhibited at an earlier period than among civilized peoples. We are told that in crying "an Indian boy is not unlike a white child, except that as a rule the Indian boy does not cry for pain, being ashamed to show that he is hurt."

The influence of age is seen also in the great frequency of the causeless cry at adolescence. The adolescent often cries for insignificant causes, or for no cause at all. Some imagined slight or even an imagined situation is quite sufficient to induce crying. The condition of luxury of grief or adolescent melancholy which usually contains a large element of pleasure, is frequently mentioned.

Twenty-seven say that the duration of the crying spell is shortened as age increases. Eleven have had the opposite experience. One observes no change with age, and another places the shortest cries at both extremes of life.

A woman of twenty-five says—"I cry less frequently than when young from similar causes ; require more provocation and recover more rapidly."

On the other hand, a girl of eighteen writes—"With years the crying spells become harder and longer ; in childhood tears were provoked at slight occasions but seldom lasted long. The child cry is in the nature of sunshiny showers."

By way of summary of the evidence in regard to the effect of age upon crying, it can be said that both the quantity and the quality of the cry change with age. The causes and conditions of its onset are different and it bears a different relation to the instinctive life as time goes on.

The early cry is predominantly one of pain, fear, anger, and hunger. Add to this the demand for sympathy and the list of causes is practically exhausted.

The question that arises with an attempt to compress the complex facts of age effect upon the cry into the limits of a simple, interpretation is whether in the development of the crying consciousness we are dealing merely with the moulding of a simple native reaction under the influence of social selection and adaptation, or, whether the complexity of the changes in the crying habit is due to the fact that there is more than a single cry motive ; whether, in a word, the expansive cry of the infant, sensuous in character, with its ego-centric foundation, and objective reference—its essentially linguistic nature—is the same in its origin, considered physiologically, biologically, and psychologically as the characteristic cry of the adult, sympathetic in character, seclusive, objectively caused and profoundly depressive and passive.

Only by a closer examination of both the mental state before and during the cry, and the nature of the physiological conditions which favor it can we hope to get any light upon this point.

The cumulative development of the cry. One of the most characteristic traits of the cry is its appearance as a crisis, the result of an accumulation of tension continued over a longer or shorter period. This development of feeling may go on after the actual crying has begun, or, on the other hand, the beginning of the cry may be the feeling crisis.

The following are illustrative:

M., 16. Must go to school; managed to keep the tears back until school was called; reflects upon the good time he might have had. "It seemed that tears long pent up now burst forth like water down a valley after a heavy rain."

F., 18. Case of personal injury, or slight; "Sad, dejected feeling that increased until I had to run away and cry."

F., 19. "When I began I knew just what I was crying about, but the more I cried the more I wanted to cry, until at last I forgot the original cause."

F., 19. "When a cry is restrained for a long time, I have noticed that in itself it is harder and more prolonged."

F., 22. Child—"begins rather quietly, but increases in vehemence and intensity. After he has fairly begun he never stops until he has cried to a climax."

F. "Have felt like crying but was restrained by circumstances; I waited under increased mental depression, till I came to a proper place, and then I gave way to my feelings."

F., 20. "I woke up in the morning with a load on my mind. . . . I dressed and went to breakfast. . . . After breakfast I could hold back the tears no longer, and went off by myself and gave way to my feelings. It grew in that I allowed myself to give way."

— — "The lump grew larger and larger until the first convulsive sob."

Twenty-nine out of thirty-one who answered the question as to whether the sob is the climax of the crying give an affirmative answer. Eighteen of the answers out of twenty-four in regard to the nature and effect of the breakdown in persons that are usually very self-controlled indicate clearly the severity of the crisis; hysteria, or physical exhaustion is frequently mentioned. This last mentioned fact shows a development of the feeling of which the cry is a partial expression. It voices the common observation that those who feel most are less demonstrative; and that feeling in those temperaments which do not allow it to spend itself in expression remains like a quiescent volcano.

Occasionally the development of the feeling appears to be rhythmical as is shown by the following excerpts from cases:

F. Child: "The crying stopped jerkily; she would cease crying for a moment, then begin again; then stop for a longer period; then another spell of crying, and so on until she finally stopped."

F. At first but few tears fell; then they rolled down her cheeks and her body shook with sobs. She would become quiet, but soon sob as before.

M. Self pity: feels himself a martyr: "so I cried again more violently than before. Tears came profusely but somewhat intermittently, swelling to great profusion as my mind turned to some new grievance."

F. Child: grief at loss of a pet dog. "She cried for two hours, and it was necessary to give her medicine to quiet her; it was only after frequent spells of crying and calm that her grief finally subsided.—Tears came in quick bursts and short gushes."

All the cases in which a cumulative development is described are cries of grief or sadness; no angry cries are so described; nor those from fear, and physical pain; the peculiar cries of childhood.

These facts indicate clearly that crying is a profound disturbance of the nervous system, that it breaks in upon a more even balance of the feelings and physiological processes as a crisis. Often the prodromal stage of this crisis, the period of accumulation, is very marked, as though the nervous mechanism was gathering force with difficulty for some act of extreme momentum. It is possible that by comparing the state of mind that prevails through the course of this phenomenon with the more general aspects of the physiological condition that accompanies it, together with the immediate and more remote physical and mental effects, some insight may be obtained into the nature of the body preparation, if it be such, which the crying act seems to involve.

Physical causes and symptoms. The relation of the bodily condition to crying is an important topic and there is much material, of great variety, in the papers that bears upon the point. A state of nervous tension that precedes the cry is frequently mentioned; reference is made to the surcharged condition of the nerves, the "nervous, touchy feeling, before going into a good cry." The increase of mental tension that precedes the cry is not accompanied by an increase of physical or motor excitement, but rather the reverse. There is an exhaustion of the nervous system, and a decreasing power of restraint. The actual outbreak comes in the nature of a sudden discharge, sometimes, on the feeling side, following the slowly accumulating tension from a single event or stimulus, sometimes as a result of a summation of causes, as in the following:

F., 21. When wrought up to a high nervous pitch, was reproved by her father for the first time since she was grown up; the result an hysterical crying spell.

Another says: "The final breakdown comes after the last straw is laid on; it comes with a storm."

F., 23. "I now have crying spells occasionally; repressed for a long time until some little thing done or said by a friend will appear to be an enormous offence, and will start me crying; attempt at restraint is of no avail until I am entirely tired out."

Among the special causes that are said to have a pre-eminent power to cause the cry the word "tired" occurred more frequently than any other: (twenty times); "pain," nineteen times; "sick," sixteen; "nervous," fifteen. In the descriptions of the cries from "inadequate causes," nearly one-fourth appear to be the result of abnormal physical conditions, half of which were designated as nervous. The person was "tired," "nervous," "worn out from worry," overwork or overstudy. The person perhaps very seldom cries except when worn out. Under these conditions the most trivial circumstance will bring on the cry; sometimes the outburst occurs even when there is no strong mental antecedent. Such crying is especially likely to take on an hysterical character. This is variously expressed. One says "Often when I am tired I have cried over things that when I am well, I will laugh at. I have cried, for example, when I have lost my point in an argument. Before the tears would cease I would usually burst out with laughter at my foolishness."

In all of the expressions used to describe a physiological cause for the cry, it seems that it is nervous exhaustion rather than muscular fatigue that is always mentioned, a condition of lowered mental tone from overwork, or similar cause. In answer to the question whether physical weakness is the cause or the result of the final breakdown in people who are usually self-controlled it is quite as often spoken of as the cause as the result. "It is usually the effect of nervous weakness that causes one to lose control." Ill health is the basis of most of the exceptions to the rule that crying diminishes with years. In descriptions of the prodromal symptoms of the cry, weakness of the knees was mentioned and assigned as the reason for the tendency to take a prostrate attitude, evidently due to a physical weakness. The relations of the nervous weakness to the cry, is also shown in the accounts of temporary and permanent melancholia in medical literature. In many of the returns the sense of the difficulty of tasks, when one is fatigued, that seems to be especially provocative of the cry is emphasized; such expressions occur as "it seemed like a mountain," "the molehill had become a mountain." Other prodromal symptoms of the cry mentioned, are pain in the back, and back of the head.

Cowles in his account of neurasthenia describes as a persistent symptom a condition that appears temporarily in the preliminary stage of the cry, and more or less throughout its course, namely, a helpless state or a feeling of incapacity. The state of neurasthenia he defines as a morbid condition of the nervous system, the underlying characteristics being weakness or inability, with languor and mental depression. The mental depression is caused by the condition of depleted nervous energy, from a sense of lack of the power to meet the conditions of life. The depression from incapacity leads to doubt and distrust of one self, anxiety and hopelessness. The anatomical basis of neurasthenia is deficient oxygenation of the central nervous system; fatigue, in its last analysis, is a deficiency of this substance in the cell. This condition is brought about by a tardy circulation and insufficient oxygenation in the lungs, or is due to sluggish circulation that fails to carry away the waste products, which consume the oxygen by entering into combination with it, at the same time poisoning the cell itself, or weakening its power to assimilate and to resist excitement.

This loss of power to resist disturbing forces means that small occasions have an unduly large effect when judged by normal standards as in the psychoses of our causeless nervous cries and our irritable people. (6 a, and b.)

Kraepelin gives a picture of a similar condition in an account of a patient with melancholia in whom there is no other symptom than a depressive and apprehensive state of mind. "The whole condition is one of permanent apprehensive depression with the same accompaniments as are found in the mental agitation of the sane, viz., a loss of sleep and appetite, and a failure of nutrition. (23.)

Richardson says of melancholy which is explained in the same physiological language, "among gestures expressing misery the most prominent and characteristic is that of weeping . . . when not fully expressed the eyes in the melancholy patient are commonly full of tears." (37.)

Certain other physical conditions clearly of a pathological kind, but not so typically nervous exhaustion, are accompanied by excessive weeping, such as some forms of acute mania, and often in hemiplegia, and in degeneration of the brain as in senile dementia. Weeping, according to Darwin, is common to insanity in general and even to the idiotic, except the cretins, even when complete fatuity is reached. (7, pp. 155-156.)

If Kraepelin is right in tracing all mental disease in which there is no mechanical lesion to nervous fatigue, and if Ribot is right in contending that pain in all its aspects, both physical and mental, is the sign of disintegration and imperfect metabolism and progressive death, then our conception of crying as a recognition of impotency is supported abundantly.

Darwin is working on this basis in showing how quickly the fatiguing effects of physical pain, fear, and the shock of sorrow are manifested. Of the last he says, "as soon as the person is fully conscious that nothing can be done, as when the mother has lost her child, despair or deep suffering takes the place of frantic grief. The sufferer sits motionless or gently rocks to and fro, circulation becomes languid, respiration ceases. All this reacts upon the brain and prostration soon follows with collapsed muscles and dull eyes." (7, pp. 79-80.) In other words, the physical attitude tends to perpetuate the physical condition, upon which the mental state depends, and when these become fixed the mood also persists in the downward progress through insanity to death.

These facts are quite sufficient to show that in the crying state the element of exhaustion or nervous inadequacy is often very prominent. The cry in many cases supervenes at the end of a period of depleted nervous strength or lowered nervous tone. Nervous exhaustion was of every degree from the effects of some minor physical disturbance which resulted in the lessening of vigor of the physiological processes, to the clearly pathological case as it occurs in melancholia, and neurasthenia.

Introspective Description of the Crying State. The typical state of mind that accompanies the act of crying appears to be despair. This is shown by the remarkable frequency of the expression of some equivalent of a desire not to live; the feeling of being helpless, hopeless, forsaken, of having no desires, is very common. This suggests at once a relationship of the crying state to such conditions as occur in the surrender stages of religious experience.

Examples follow:

F., 18. "I felt that no one loved me, or cared for me in any way; I felt that even God himself had forsaken me."

F., 21. "I felt discouraged; what is the use of trying any more. I had but one wish, and that was thwarted. Didn't care whether I lived or died and rather preferred death."

A feeling of injustice and a corresponding resentment towards life as a whole is often expressed in the language in which these mental states are described. Persecution and self-pity come in strongly.

—— "I wanted to die. I wanted to get out of this cruel world. Hoped to get sick to make my parents feel sorry."

"Wished to be sick but not to die, for then I should not be able to see those suffer who had made me suffer."

The state of mind that accompanies the cry is extremely autosuggestive, the stream of consciousness, when it is not clogged, is likely to take the form of imaginative inference, to an almost paranoic degree. The subject imagines unfortunate situations and disasters that have little relation to reality and these in turn add to the depression, if they do not serve to distract the attention.

Seclusion is usually sought when the crying begins; this tendency seems to be a more deeply seated impulse than the mere desire to escape the taunts, comments, or sympathy of friends. There is a strong realization of the desperateness of the situation for the moment, to the extent that sympathy is oppressive; the crier desires to be alone; it is apparently only in the cries of the infant and the young child that the cry is toward some one, and then the state of consciousness seems often to be a divided one, divided between the call for assistance or permission to break up the cramp of the will, and a desire to be let alone.

In a word the state of mind in crying as typified in the cry of grief is one of hopelessness; there is a cessation of the will to live; the hopelessness becomes active to the extent of filling the consciousness, sometimes to the exclusion of the cause and the situation to which the cry immediately refers, the mind goes on to dramatize death and total renunciation of effort. There is no call for help as in the child cry, but a realization of the hoplessness of help and a sense that the internal forces are inadequate to rouse the will from its paralyzed state. In all of the forms of crying there is probably an element of sadness. When a girl cries because she is "beaten in an argument," or when a man cries when he is about to go into a fight, there is presumably an element of apprehension and discouragement. The state of unmixed anger is not a tearful nor a hopeless state of mind. If we follow the angry, tearless cry of the infant to the period when the vocalization is inhibited by social disapproval, this is distinctly seen. The man who shoots his rival in love or business is not in a crying frame of mind; neither is the Malay who runs amuck killing every one he meets until he is himself destroyed. Hatred, envy, sullen rage, do not take on this form of expression. The dagger, tooth and claw are not symbols of crying. Anger can be defined as a pleasure in inflicting pain. Biologically we have in anger a condition of action in which vision cannot well be blinded by tears. Even in hatred, jealousy, and envy, which are called passive forms of anger, the keenest vision and the highest degree of sense alertness are demanded; with it a soberness of mind typically expressed in generalship and financial competition. So too in the war *cry* of the savage, the conditions of the crying state are absent; the word "cry" here is employed in a derived sense. The feeling here is not one merely of anger, but expresses an idea of social action quite the opposite of the true crying state. In other ways the word cry has come to be attached to states that are related only symbolically to the crying state. In the "cry" of the auctioneer and the street pedlar, the metaphorical and symbolic meaning of the word and its remote connection with the idea of appeal is clearly seen.

In the cry from fear there is the element of helplessness and sorrow as we see well in the derived and mixed fear states such as apprehension and dread where there is the element of depression and diminished vital and motor action. The same is true of physical pain.

In the cry of joy, there is a mixed emotion, attended with intellectual elements. Those states in which laughter and crying alternate are similar in nature, as when one is severely tickled or tantalized. The cry from joy, as when the girl meets her parents after a long absence, contains the element of hopelessness through association with other elements, and the mind goes back to the time before it was emancipated from the strain of the, for the time, irremediable loss. Possibly the sense of the loss is heightened by contrast with the present relief. In the moment of any great success the memory of the severity of the struggle toward the end is probably present to supply the needed mental element to construct a crying situation. In the cries of joy there is a sense of being helpless, conditioned, or carried away by the force of a power that is beyond the control of the individual and heedless possibly of our safety or pleasure. This is especially evident when overcome by a sense of the sublime. The alternations of feeling help to raise the tension and nervous excitation. Where in this alternation the joyful element preponderates we have the laughing cry instead of the crying laugh. The two conditions are identical, and the physiological result at any moment is due to the dominance of the one or other of the states of pleasure or displeasure. In both, the previous conditions are similar, namely, a strained, serious, anxious state of mind.

Crying, if our analysis is correct, is essentially a condition that, though varying much in form and expression, is one in the sense that it always contains the common element of sadness, helplessness, or hopelessness. The cry is the physical accompaniment of a mental state which is a recognition of an inability to remove certain painful or oppressive conditions; the cry appears when the feeling has reached a certain intensity. We must remember that pain, if we accept the present verdict, is connected physically with a state of disintegration whether this be in the form of temporary painful state or the fixed conditions of melancholia and other mental abnormalities. It seems likely that all painful states of mind, when the intensity is great enough express normally, themselves in crying. The cry is a physical sign, writ large, of the insufficiency of the organism and primarily an appeal for help from without; in its most primitive form, a recognition that destructive forces are dominating and that the fighting chance has gone. The will to live has ceased and the whole organism is in a dying state.

The child's cry is largely an appeal to outside sources and is best described as a state of helplessness. In a deep stage of grief, better shown in the adult, the helplessness becomes hopelessness, the individual gives up the struggle to adjust himself to the demands that are made upon him. This cry is characterized by quietness of expression and seclusion, and a prone attitude of the body, suggesting the abandonment of life, which is common among many gregarious animals when they are overcome by disease or weakness and are destroyed by their kind, and the custom among nomadic savages, according to which the old and injured are disposed of, or voluntarily leave the horde and go away to die. Thus does the sorrowing human in modern life suggest the state of mind in which, to be burdened beyond the power of self to throw off the incumbrance, is to be worthless and socially unfit to live. The many suicides in grief, which are but a positive expression of the state that essentially exists in all mourning, illustrate the nature of the crying condition. The holding of the breath so common in the cry of anger is possibly rather an instinctive than a voluntary expression of the widespread cessation or inhibition of the vital processes that occurs in this state.

Co-ordinate with the deeper seated physiological stagnation in the condition of sadness that we have called a dying state, a dying state of mind is shown in the surrender of the processes of mental adjustment, quite in keeping with the slowed circulation and weakened condition that occurs in all stages of genuine depression, whether normal or abnormal.

We are now in a position in regard to the facts of crying to construct a partial summary. Crying appears to be a reaction that occurs in connection with many apparently diverse states, such as anger, grief and joy, and a provisional classification has been made into these three classes. Each of the forms proved to be widespread, and, so far as could be determined, universal among the races of mankind. Certain age differences affecting the cry came out clearly; the child's cry is more often active: it is noisy and plainly directed outward, and for the most part seems to be the result of inner needs. The child cries more often from anger, the typical adult cry is from grief; it is quieter, tends to be subjective, the victim seeks seclusion. Its occasions are more complicated. The child cry is essentially an expression of helplessness, largely in regard to bodily needs and desires: the cry of the adult is more an expression of hopelessness. There is the feeling of quite as much need, but the cry is no longer directed outward for assistance.

The cry is a profound disturbance, a large reaction, and no mere shedding of tears. It occurs as the end phenomenon of a cumulative development of feeling, a physical dualism which is characterized by a low stage of energy, or a stagnation of activity, accompanied by a condition which requires an effort out of proportion to the power of the organism. In the typical adult cry this schism between the power and the need increases until the outbreak of the cry comes. Whatever else the cry may be, it is a cessation of the state of strained effort, in the direction of a total giving up. The state of adjusted motor co-ordinations and attention, which the individual maintains normally in all situations of life entirely ceases; it may be said that in place of a definite adjustment of nervous mechanism a state of wide diffusion quite the opposite of adjustment supervenes and movements are no longer adaptive. Both in the crying state itself and in the antecedent periods of tension and depression, the condition is one which suggests death, in the sense that it is a cessation of normal function. The will maintains the struggle after the disintegrative processes of pain are expressed in the physical inadequacy to respond to the demands of the situation. All cries can be reduced provisionally to this typical form.

The presumption is strongly in favor of the view that a reaction so uniform in its characteristics and so widespread over the human race is instinctive in its actions. Doubtless it has been preserved in the race because it breaks up a condition of strain of adjustment, which is kept up in the face of pain, to the exhaustion of the nervous energy. Pain is the first warning of the need of the organism to cease effort. Crying acts as a second but more uncontrollable interruption of the authority of the will, when, as it were, the body takes the situation to itself and prevents the further continuance of the effort. Whether this act is in any other respect than as a mere interruption, a beneficial act, remains to be seen.

It remains to study the cry as a series of definite, particular physiological and motor events, in order to discover if possible why under situations such as have been described the organism reacts with this particular, very remarkable complex. Further light may be thrown upon the question of the instinctive nature of the cry and the significance of its elements, by a study of its effects.

II. THE CRYING ACT; ANALYSIS AND EFFECTS.

In the previous chapter some conclusions were reached about the relation of the outbreak of crying to the causes and conditions under which it occurs; it now remains to examine the actual event with reference to analyzing it into its elements and to study each of these as a physiological act, with two questions in mind; first, the genesis of the particular form which the expression of grief has taken in the human race in the act of the cry, and second, the effect of this act which has led to its widespread adoption or its preservation as a means of expression. The first chapter had to do largely with the question as to why we cry; the present chapter with the question why we cry just as we do.

We shall find that the explanations that have been suggested and the conclusions from the present data lead to a classification of the explanations of crying into two groups, the first, mechanical or physiological, the second, biological. Each covers, in a way, the questions as to the reasons for the particular events that occur in the cry and its preservation in the race, on the ground of its effectiveness. It may transpire that these views are not entirely antagonistic to each other, but represent partial truths.

The crying act, as was said above, is a complex event, a series of motor and physiological changes, that sweep over the nervous system under certain conditions. Crying is not merely a shedding of tears nor is it entirely expressed in the changes that appear externally. It is very widespread in the body, and involves obscure as well as more obvious events. But the whole process, both in its internal and external aspects, cannot be ferreted out in our present state of physiology.

Starting with the answers to the questionnaire as a basis, a provisional grouping of the changes that occur in the cry act can be made as follows:

(1) Changes in the circulation, (2) attitude of the body, (3) lump in the throat, (4) vocalization, (5) sob, (6) tears.

The complexity and wide range of the cry can well be shown further by a brief statement of the common physiological information in regard to the nervous mechanism that is involved. The act of crying is due to action of the fifth, seventh, ninth, and tenth cranial nerves together with the superior cervical sympathetic. The secretion of tears is due to stimulation of the fifth and sympathetic, the alternate paling and flushing to the sympathetic, while the consequent general stimulation of the respiratory centre results in the sobbing and general convulsive movements of hard or continued crying. Various other nervous phenomena, such as cold hands, are due to disturbance of the sympathetic system. When the anatomical relationships of the tenth nerve are recalled, the various vague body sensations that accompany crying can readily be understood as also the profound depression that so often follows prolonged crying, for the pneumogastric is thought to have a "depressor" cardiac branch. It is certain, at any rate, that, in the lower animals, strong or prolonged stimulation of the pneumogastric stops the heart. Some of these physiological facts will be discussed more in detail later. But this will be sufficient to show the nature of the nerve mechanism which must be taken into account in a description of crying.

Circulation. Altogether a disturbance of the circulation has been mentioned fifty-four times in the returns. Thirty-five speak of a change in the complexion; twenty-five have noticed redness of the face; three mention paleness. A black or purple color, in the case of children, is described. Others speak of the red face and cold hands; hot hands are also mentioned and the fact that people differ much in the circulation accompaniments of crying. The changes in the rapidity of the circulation also appear to differ from case to case. Ten writers express the belief that the circulation is accelerated: eight of the number seem to have inferred this from the redness of the face; but this does not necessarily follow.

The indefinite expression "increased heart beat" occurs; there are "throbs in the head." Another says more definitely that the heart has been retarded in rate but beats more violently. One other observer has recorded a retarded circulation. Other references to circulatory changes are made in describing the after-effects of the crying spell; such as bloodshot eyes, the mottled complexion, parched and distended lips, and burning eyeballs.

While these returns do not allow conclusions in regard to the exact changes that occur in the circulation, the variety and generality of the vaso-motor disturbance is clearly indicated.

Attitude of the body. In tabulating the returns for the purpose, it was clearly shown that in the cries of anger, fear and pain, there is a larger amount of body movement and facial contortion than in the grief cry. These movements in the angry cry are very diffuse. In the one hundred and twenty-two descriptions of the angry cry, forty-four references were made to the throwing of the limbs, kicking or striking in an aggressive manner or bumping the head upon the floor. One writes that in the angry cry there is not a feeling of weakness or despair, but a feeling of strength or desire to fight. In pain there is a writhing of the body and throwing of the limbs. With these movements goes the suspension of the breath and resultant black or purple discoloration of the face, a condition limited to the cry of pain and anger.

In the cry of sorrow, on the other hand, there is the attitude of lassitude, especially preceding the spell. One speaks of a feeling of weakness during the cry; another of such exhaustion that she "can't stand up." Feeling of "weakness in the chest," "utter weakness, especially in the knees," are mentioned, also a tendency to the prone position of the body as characteristic of the cry of sadness. The prone position was mentioned forty times and the "erect" but once. The fact that lying down increases the tendency to cry, and negatively that "one gets over the crying spell more quickly if one remains standing," is brought out in the returns. It was noticeable, also, that the vocalization disappears in this form of the cry. Even in the child the vocalization is subdued in pitch and becomes the plaintive moan or wail in the sadness cry. The influence of the prostrate position of the body in quieting a child is mentioned and the fact that one cannot indulge in the abandoned cry while in a standing position.

The historical and ethnological material is full of reference to the bodily attitude in the cry. To give a full account of this topic would necessitate describing mourning ceremonies among various peoples. Of these there is a great variety. The grief expressions and ceremonies as described in the Bible illustrate the manner in which, to the common elements of movement and attitude which express grief, various ritualistic elements are added, and the manner in which grief becomes formulated and conventionalized. We read of the "virgin girded in sackcloth and ashes for the husband of her youth." (Joel 1: 8.) Ahab in penitential mourning "rent his clothes and put sackcloth upon his flesh," like a man sorrowing for the dead. (I Kings 21:27.) In a wall painting of an Egyptian tomb a funeral procession is represented with the master of ceremonies "followed by eight women and four men, . . . all making gestures of mourning, by beating their breasts and their mouths while wailing, or by throwing dust on the head." (16.) To tear the hair and the beard (Ezra 9:3); to put sackcloth and torn garments upon the body (II Samuel 3:31); to sit among the ashes (Job 2:8); to sprinkle earth or dust or ashes upon the head (II Sam. 13:19) were actions in which sorrow and grief more or less naturally or conventionally expressed themselves. . . . . To go "bowed down heavily" (Ps. 25: 14); to go barefoot and bareheaded and to cover the lips (Ezek. 24:16, 17; Mic. 3:7) were less demonstrative tokens of mourning. Mutilation of nose, brow, ears, is mentioned by Herodotus (4:17) as being practiced by the Scythians in token of mourning for a departed king. Such mutilations were forbidden by the law of Moses. (Lev. 19:28; Deut. 14:1), although we read of making bald the head and cutting off the beard (Isa. 15:2) and even laceration of the body as a sign of vexation and grief among the Israelites. Among the Arabs it was customary for the women both to scratch the face until the blood ran and to cut off the hair (16).

The following refers to the Kiowa Indians: "The men also have their hair cut off at the shoulders, and had discarded their usual ornaments and finery. On one occasion while driving near a camp we were attracted by a low wail, and going to look for the cause of it we saw sitting in the tall grass near the roadside a bereaved father, stripped to the breech cloth, his hair cropped close to his head and blood dripping from gashes that covered his naked body. He did not look up nor turn his head as the wagon passed, but continued the low wail with his eyes cast upon the ground." (29.)

An ancient custom among some North American tribes was of a woman, on occasion of the burial of her husband, to cut off a portion of a finger (one or more joints) and have it suspended in a tree above his body. (49, pp. 109-110.)

The great mass of this testimony goes to show that the expression of sorrow, as developed into rites and ceremonies, is made on the basis of self-torture and abasement. The positions that are assumed are those of resignation, abasement, humiliation and neglect or active abuse of the body. These motions of withdrawal and prostration are in keeping with the natural expression of grief as it appears in a civilized child and adult in the prone position; the rite is a voluntary attempt to carry further an instinctive reaction. In a professional mourner the expression of grief has become conventionalized and he performs for all, the acts of self abasement which are the essence of the rite.

Vocalization. In childhood, before vocal language has been developed, the vocal cry is essentially an instrument of expression. This is well brought out in the returns and confirms the facts already conceded by students of language. The specific cries of hunger and pain were noted and the fear cry and the cry of anger or "spunk" are also mentioned. The child "tells" when he is hungry, cold, or in pain by his cry. (7, p. 91.) It is recognized that in the vocalized cry of the infant he is calling attention, and that the cry of the older person is for the purpose of giving "vent" to the feelings, and that as one gets older the vocalization is "suppressed."

More specially to describe the vocalization in the old and in the young, such words as "shrill," "a howl" are used in characterizing the cry of the infant and the young child. Compared with the adult's it is more "vehement," "louder" and "higher." The cry of the child is thought by some to be intentional, while that of the adult is repressed. In describing the vocalization in the cry of "old and young," such words are used as "harsh," "scream-like," "emphatic," "shrill," "a wail," "sharper," "louder," "more prominent, but with less of feeling," are used with reference to the latter. On the other hand, the absence of vocalization is frequently mentioned with reference to the old, and when it is mentioned it is usually spoken of as subdued, quiet, of a lower pitch, suggesting a state of feeling of greater depth. It is "muffled," "faint," "restrained," "a gurgle," "moan," a "sniffle," "weak, but with more tears," "more subdued," "more quiet," "less loud," "more feeble but more heart-rending," "more heavy, and deep because controlled," "consists of sobs and gasps only."

The voice is mentioned in but seven of the actual descriptions of the adult cry and in the replies to other questions. In one of these it is said that the vocalization is produced by the attempt to speak, in two others it is characterized as harsh. That, however, in the adult the vocalization tends to return when the grief is extreme, is suggested by the returns. The restraint that is exerted upon the adult cry by the presence of others is mentioned, and the fact that when grief is extreme, this is overridden and the cry takes on more of the infantile character.

Coinciding with the unmistakable fact that vocalization is more pronounced in the cry of the infant and young child, is the evidence of a similar distinction between the cry of the savage and the cry of the more civilized races. For example it is said of the Esquimaux about Behring Strait, that "when a person dies during the day, his relatives amid loud wailing, proceed at once to dress him in the best cloth they possess," etc. (46, p. 272.) Of the Sia, a tribe in New Mexico, we read: "The immediate relatives in consanguinity and clan, are present during the bathing of the body, and make the air hideous with their lamentations" (43). Of the Sioux, it is said that after the death of the warrior, the widow cuts her hair, mutilates her body with a sharp flint, and meanwhile "keeps up a crying and wailing." "After depositing the body . . . they continue exhibiting their grief, squaws by hacking their arms and legs with flint . . . the men would sharpen sticks and run them through the skin of their arms and legs, both men and women keeping up the crying generally for the remainder of the day, and the near relatives of the deceased for several days thereafter. . . . Similar mourning was kept up at the death of women and children. It is not unusual, at the present time, to hear a man or woman cry and exclaim, 'Oh, my poor wife, or child,' as the case may be, and upon enquiry to learn that the death happened several years before." (49, pp. 109-110.) The males of the Heada and Tlinget tribes were said to have "childish fits of crying." The cry of the Samoan women was spoken of as "very noisy;" and by another writer the crying of both sexes of the Samoans was described as "natural and childish." The word childish was also applied to the crying of the Hawaiians. Of the Yo-Kai-A Indian it is said "The chief of the visiting tribe made a brief speech in which he no doubt referred to the death of the chief of the Yo-Kai-A, and offered the sympathy of the tribe in their loss. As he spoke some of the women scarcely refrained from crying out. I presume that he proposed a few moments of mourning, for when he stopped the whole assemblage burst forth in a bitter wailing. Some screamed as if in agony. The whole thing created such a din that I was compelled to stop my ears. The air was rent and pierced with their cries."

Free vocalization in the cry is abundantly attested in the Bible as having been practiced among the Hebrews. This is noticeable even in the cry of joy, for "Jacob kissed Rachel and lifted up his voice and wept" (Gen. 29:11). When Esau heard the words of his father, that Jacob had secured the blessing originally meant for him, "he cried with a great and exceeding bitter cry" (Gen. 27:34). On seeing the self-sacrifice of his brother Judah in offering to sacrifice himself for his brother Benjamin, Joseph was touched, "could not refrain himself before all that stood by him . . . and he wept aloud." (Gen. 45:1, 2.)

Other evidence suggests that as the cry becomes formal and ceremonial the depth of the feeling may not be in very close relationship to the intensity of the expression, that it is the deeper feeling that is naturally related to quiet crying and tears, that naturally the expression is vocal only when the tears are insufficient as a safety valve to the emotion. This loud wailing of the savage is probably often unaccompanied by tears, as in the case of the Kanaka, of whom it is said that "he cries to the utmost extent of human endurance, although tears do not flow so freely as the grief and sobs would indicate." Another writer describes the crying of these, as loud and easily repeated on slight occasions. "Between the arrivals of the friends of the dead, those already present indulge in laughter, and feasting. The grief at the public funeral of the Maoris was described as only partly real. It was marked by a long mourning wail which was "all over as if by magic; after which they indulged at once in feast and laughter." The Samoan's grief is "soon forgotten." (From missionary returns.) Of the Yo-Kai-A, Stephen Powers said, "at a given signal they ceased weeping, wiped their eyes, and quieted down. Then preparations were made for the dance" (49, p. 193). These and other facts indicate that among savages the crying act is largely built upon by custom and ceremonial. Crying becomes a profession in the mourner. The "louder" they can cry the "more expert" they are regarded, as among the Hawaiians.

Some differences in the vocal cry of races have been carefully described by Mantegazza. Among the Niam-Niam it is "Ow, ow" to "okoun, okoun," in intense suffering; among the Bongos, "aoh," the Dyor "awai, awai," the Papuans of

New Guinea, "ac," the peoples of New Hanover "se, se" (27, p. 259). The Samoans, according to our returns, cry "aue, aue." The quality of the intonation has also been mentioned in the ethnological returns. The close relation of the cry to musical expression is also indicated. The dirge of the professional mourners among the Hawaiians, is said to have a most touching character, and "to follow the scale as accurately as our musician does his song." Among the negroes the vocalization of the cry takes the form of the funeral moan or chant. Among the Indians tears are attended with a song recounting the good qualities of the deceased.

The data in regard to vocalization in the cry indicates the essential oneness of the crying state. The natural expression of the crying state is the vocal cry, which is a call in a state of helplessness. This primitive fact becomes modified in many directions, and, like all useful expressions of emotion, is highly developed. In the adult cry of grief we find the vocal cry suppressed, but that it is a suppression is indicated by the fact that it occurs again when the state of feeling is intense. The savage as an adult not only freely expresses the mental state of grief by vocalization, but makes use of it as a ceremonial, and voluntarily increases it just as the child does when he cries voluntarily for attention or sympathy, but for quite other purposes. In the "hopeless" stage of the adult cry, the cry is no longer for help and our customs place a ban upon loud expression of grief. Observation leads to a conviction that there is considerable difference in this respect in the expression of grief in the higher and lower classes in our own communities.

The lump in the throat. The lump in the throat has been so commonly referred to in the returns as the first symptom of the coming cry, that the fact must have some significance though the returns are not sufficient to establish the point beyond suspicion. In answer to the questions in regard to the nature of the lump in the throat as felt, its repression, its effects upon the voice, respiration, complexion, and circulation, the following characteristic phrases were used:

"It comes just before the cry; cannot swallow;" "pain or ache in the throat;" "feels like swallowing the larynx;" "increases with the attempt to swallow, unless the mouth is opened and the face is straightened into its natural position;" "it may be suppressed."

The effect of the lump upon the respiration is indicated in such expressions as "makes it difficult to breathe," "almost choked." "It will move neither up nor down." "The throat feels full." "The breathing is hard and strained." Twenty-two of the thirty-six answers relating to the respiration declare that the breathing is made more difficult. In addition to these, there are seventeen references to the loss of breath in the crying spells of childhood. Thirteen of these refer to the cry of anger, and four to fear.

Forty-eight have noticed a disturbance of the voice, while only one declares that there was no such disturbance. One says, "It feels as though something came up into the throat and stopped the respiration and the vocal organs, a kind of catch in the throat, making breathing difficult and speech almost impossible." Another writer says, "A hollow aching lump seems to rise in the throat until in the back part of the mouth; this quickens and shortens respiration and causes the voice to be weak and broken." It "cracks the voice," "hinders respiration," and sometimes "entirely cuts off the voice." The words, "guttural," "husky," "harsh," "choking," have been used to describe the nature of the disturbance of the throat. Four have noticed the increase in the lump in the throat when the effort is made to suppress the tears. Others say: "The tears relieve the lump;" "must cry to get rid of the lump." "The lump is more noticeable if one is with people and tries to suppress the tears—" One gives an explanation, "The feeling is caused by the tightening of the muscles of the throat to prevent the convulsion in the breath, that naturally takes place when one cries." The progressive development of the lump, and its relation to the sob, has also been observed. "It grew larger and larger until the first convulsive sob: then it seemed to stop." With another the lump continues throughout the cry. Four are unacquainted with the "lump" sensation; one thinks that it is not always present; another "has not felt it since childhood."

The data in regard to this interesting symptom of the cry are insufficient to warrant a physiological interpretation of it. That it is connected with disturbance of breathing and vocalization appears certain. On the other hand it is connected with movements of swallowing, and the relation of it to disturbances of the mechanism of the digestive tract is indicated.

The Sob. In answer to the question "What is the sob?" the following definitions were given: "a convulsive catching of the breath," "a convulsive choked drawing of the breath," "a natural tendency to hold the breath in extreme grief;" "one makes little gasps for air; these are the sobs;" "a convulsive sigh;" "a sob is a short, quick sound, which indicates the cessation from prolonged weeping." "It occurs at the end of the spell and lasts for some time." Other definitions are "a convulsive contraction of the diaphragm," "a convulsive drawing of the breath," "a sharp drawing of the breath," "a choking sound made in the throat, accompanied by a convulsive movement of the shoulders, or of the whole body, coming at regular and frequent intervals;" "a short and rapid inhalation, two or three for each exhalation culminating in a fit of gagging and grimaces, and gradually diminishing in rapidity to about one in twenty-five seconds." Another records the fact that the sobbing is more rapid at first, perhaps five or six in a second.

Other remarks refer to the control of the sob. "The sob is the most difficult part of the cry to control." "Outside influence can have an effect upon the other symptoms but the sobbing continues."

The relation of the sob to the vocalization is indicated. "A deep and agonizing breath suddenly expelled and another deep breath taken." The sob is thought of in three cases as the vocalization of the cry. The expression "sobbed aloud" occurs.

The late appearance of the sob and its relation to the shedding of tears have caught the attention of the observers. "The sob continues later than the tears." "It generally follows but may accompany the tears." "It comes after the tears." "Tears give way to sobs;" "most noticeable after the tears have ceased," at the end of the cry. "I always sob for a long time after the tears have ceased." Another, speaking of the order says, "the tears cease to flow, the lump begins to disappear, the sobs slowly cease." Preyer says, "not weeping but the sob comes late."[1]

The order of events in the cry of a Japanese child was especially observed by Dr. Theodate L. Smith. At the 7th week it was as follows: (1) Drawing of the mouth to a square shape (in the 12th week a protrusion of the lower lip preceded crying); (2) closing of the eyes; (3) the vocal sound â â â; (4) reddening of face; (5) tears. The crying was not accompanied by snuffling, and there was no indication of the sob as late as the 7th week, except its possible beginning in a slight catching of the breath. In the 9th week sobbing was clearly present.

The reddening of the surface that accompanies the cry was definitely observed in the 9th week. It began in the face, spread up over the top of the head, and simultaneously down toward the feet.

The early appearance of tears in this child (almost from birth and certainly within the first forty-eight hours, according to the records of physician and nurse) is exceptional.

As early as the 7th week the vocal cry was well differentiated and the cries of hunger, pain, discomfort, sleepiness and anger clearly distinguishable from each other.

Observations in regard to age differences—"In the child the sob is usually the climax; in the adult the cry often begins with the sob." Another describes the typical adult cry "as sobs and gasps only;" or "only sobs, tears occurring but occasionally." These examples suggest that the sob plays a larger part in the crying of the adult and appears earlier in the crying act. The explanation is that the sob persists at a time of life when other elements of the cry have been inhibited. The sob also varies with the nature of the feeling which accompanies the cry. Ten have noticed that sobs accompany the angry cry but four have also mentioned that there is less sobbing than in the grief cry. One reports that the sob is present in the cry of joy.

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[1] Some observations upon the development of the sob and other points that were received too late to incorporate can be mentioned together here.

The sob appears to be absent in the young infant, and its first appearance is recorded at about the time when tears also appear, not perhaps until about the end of the fifth week. The sob appears to be an entirely human manifestation of grief. Darwin records that the "monkeys scream and pant but do not sob." (7, p. 157.)

There is little explicit mention of the sob in the ethnological returns. Its absence has not been observed among savage peoples, and its presence seems occasionally implied, as in such expressions as "a perfect outburst of uncontrollable grief" in the case of a male Indian. That some, at least, of the Indian tribes sob is shown in the above (49, p. 103) referring to the Yo-Kai-A tribes of California, which says that when a visiting chief referred to the dead chief of the tribe, "with difficulty they suppressed their sobs." Among the Sandwich Islanders, the sob is said to be pronounced. The sob is probably universally, as well as distinguishingly human.

According to these definitions and descriptions the sob consists (1) of a deep and violent inspiration, opposing a tendency to choke. It seems to be due to a spasmodic contraction of the diaphragm, the choking being caused by a partial closing of the glottis, and constriction of the vocal cords. When the sobbing is most violent, the interruption may break the inspiration into a series of broken fragments and a gasping sound may attend the vibration of the vocal cords. A like spasmodic movement, though less pronounced, may attend the effort to expel the air, the air passages continuing to remain constricted. The term sob is often used as including this movement of expiration. This phase seems to be the last stage of the vocalization, the remnant of the unrestrained "a-ha-ha" of the child cry.

It is probable that the breaking of the rhythm in the respiration belongs both to the inspiratory and expiratory movements. Expiration is less under muscular control; the catch in the inspiration is partly at least an effect of broken muscular innervation; so that just to the extent that the expiration is muscular it partakes of the jerky character of the whole muscular process.

The sob appears to be a later symptom of the cry persisting after the flow of tears has ceased. In a case recently observed, the child was seen after tears had ceased; the marks were still upon the face, vocalization had also discontinued, but he was still sobbing violently with a movement much like hiccough. This appears to be typical. The late appearance or the persistence of the sob as an element of the cry is clear enough but the time of its appearance is less easily determined. The evidence is not conclusive but it is indicated that the sob appears after and relieves the lump in the throat. Yet in some cases it was observed to be present, at least, as a tension of the diaphragm, when the eyes were merely suffused with tears, and the sensation in the throat scarcely noticeable; it was then apparently simultaneous with these symptoms. It may not only persist after the tears have discontinued but it may appear in a paroxysm that involves no shedding of tears at all. The sob seems to be essentially human, though it is not necessary to the crying spell; tears may flow without it. It seems to be characteristic of the climax of the cry and is probably always present when the paroxysm is intense. It seems to be always absent in the cries of animals.

These points are still further involved by the fact that in the adult and in cases when the stimulus is intense, the crying may begin with the sob. It is quite possible that there is always in the cry a disturbance of the respiration, but that it does not always appear either to outside observation or to introspection, unless it is pronounced.

There is no certain testimony in the returns as to the location of the beginning of the convulsive movement of the cry. Twenty-five observe that the throat is prominently affected. But some other part of the body is usually also mentioned as prominently affected. The diaphragm, the chest, abdominal walls, stomach, lungs, thorax, or the abdomen as a whole are repeatedly referred to in the papers.

Some anatomical explanation for these varied phenomena is found in the wide distribution of the pneumogastric nerve, and its intimate connection with the solar plexus of the sympathetic system.

Tears. The young infant does not shed tears, and the age of first appearance in sufficient quantity to be seen upon the cheek varies from two to five months, according to different observers. According to Darwin's observations, they first appeared at 139th day (7, 153). But he quotes others who place this date at 20, 84, 110, 104 days. Preyer observed tears on the 23d day (33). Perez places this onset at between two and three months (32, p. 66), while Mrs. Moore has observed their appearance as early as the sixteenth week (30), Miss Shinn at one month (41), Lowden, at 53 days.

After their first appearance they remain throughout life connected with the typical cry; they may remain as the only visible symptom of the cry. In regard to the time of appearance of tears in the paroxysm, the returns are not conclusive; fifty-five say that they are the first symptom to appear; sixty-three say that the lump in the throat precedes.

The proportion of the entire spell over which tears continue is also not definitely determined. There appear to be individual differences. In some cases tears cease early in the spell; in others they persist. But all who mention the point maintain that the tears cease before the end of the crying as a whole. One says "about two-thirds of the whole period;" another speaks of the dry sob remaining after tears have ceased.

Running of the nose during crying is mentioned in some cases. This indicates that a portion of the tears have passed down the nasal duct instead of overflowing in the eye.

The rate of secretion of the tears appears to be another variable characteristic; in some the rate is said to be faster at the beginning of the cry; others say that tears increase as the crying continues. Excessive shedding of tears seems to be an idiosyncrasy. They appear to be much more easily stimulated in some than in others, perhaps quite independently of the amount of feeling. Instances are given by the correspondents: as, for example, a man who always sheds tears when he reads anything that is in the least pathetic. Altogether twenty-eight cases were mentioned among the returns in which there was said to be a tendency toward excessive shedding of tears. The absolute capacity of the glands is sometimes very great. Darwin tells of an idiot who cried the entire day on discovering the æsthetic blunder of having shaved his eyebrows to promote their growth. (7, pp. 155-156.) Other cases of long continued shedding of tears were mentioned in the returns. One had the eyes continually suffused with tears, so that it was necessary to repeatedly wipe them away.

Tears are but little under direct control of the will. Three-fourths of the answers to the question as to whether tears can be repressed, contained the statement that they cannot, or gave conditional answers. One says, "only for a time." Another, "only when the feeling can be conquered.' "It is impossible when one is alone or when the influence of society is wanting." Similar conclusions are reached by the observers in regard to artificial crying. In this connection it was said by fifteen of sixteen observers that the tears are the hardest part to manage. One says "Unless I am really sorry, I can never shed tears." One found that, in trying to cry, she could sob, and bring the tears to the eyes but could not shed many nor get the peculiar sensations of a real cry. Others seemed to manage better and one relates that "if the face was contorted, tears would flow and a feeling of depression would follow." Another: "I took part in a play in which I was obliged to feign crying. After a little I began to sigh and moan, and in a short time I was really crying." A well-known actor was asked how he managed to weep at will, and replied, "I call up the image of my dear father, who is dead."

It is likely that most of the instances reported are cases of manipulation of the feelings rather than direct control. All the accessory efforts are probably merely mechanisms that result in conjuring up the sad feelings.

The effort to excite tears deliberately implies their intimate relation to a depressed feeling since their effectiveness consists in exciting a sympathetic feeling in the minds of those to whom they are addressed. That "tears are woman's most effective weapon" we are continually reminded in our returns. All except two of fifty writers who have described the effect of tears of parents, teachers, and friends, upon their conduct are agreed upon the great effectiveness of tears to conquer a stubborn will or subdue passion, when everything else fails. "They caused sorrow and regret over my misdeeds." "They affect me when nothing else will." "I never saw my mother cry but once and that moved me more deeply than anything else in my life." "When my parents or friends cry over my conduct I feel ashamed of myself, and make resolves to be better in the future."

Tears, although, of course, common in both sexes, and more common in women than in men, are possibly more frequent in proportion among the males in some races than in our own. The crying of both sexes has been especially mentioned in writers about the negroes, Indians in the Heada and Tlinget tribes, the Japanese, the Samoans, the Sandwich Islanders, and the Maoris. The evidence is strongly in favor of the view that tears are more frequently shed among the lower races of mankind than among civilized peoples. A Maori chief cried like an infant because certain sailors sprinkled flour on his choice clothes (7, p. 155). Children also shed more tears than adults; it seems to be the current opinion among the correspondents that tears "last longer" in childhood, but there is no conclusive evidence. Some who have answered the question have doubtless been led astray on this point by inferring that because children "cry" more they shed more tears. Many of the cries of the young child are vocal in their nature and tears are entirely wanting. This is especially true of the angry cry. Of the angry cry it is said "the flow of tears was not very great." Of twenty-nine who have referred to the tears in the angry cry, fourteen observe that they are less frequent, and eight declare that they are characteristically absent. In two cases of the five in which tears were mentioned in connection with the cry from fear, the tears were said to flow only after the danger was over. That the active form of the cry from fear is tearless, is suggested by the answers to the question about the inhibitory effect of fear upon the cry. Fourteen out of twenty-one say that fear inhibits the cry. One says that intense fear inhibits crying; another that if fear is not too intense, crying will result. The transition from this upper limit to the tearful level is indicated in such replies as the following: "Fear is likely to cause numbness at first, and then tears." In general the active element in the emotional state inhibits the tears though it may not have any effect in lessening the vocalization, and may increase it. In fear, for example, the more intense form is one of inhibition of both movement and the cry, then at a little lower level screaming predominates, and in the fear that is more largely representative, tears are more common and they may flow freely after the shock of the fear is passed.

Intensity of emotion, even of sadness, seems to have the effect of inhibiting the tears. Some report that they cannot cry when they feel the saddest, which evidently refers to the tears, since other elements of the cry are more under the control of the will.

The shedding of tears from grief has also been noticed among the higher animals. Darwin reports that "the Indian elephant is known sometimes to weep . . . . when captured it sank to utter prostration, uttering choked cries, with tears trickling down the cheeks," and again "the female when distressed by the removal of her young one contracted the orbicular muscles when the trumpeting began." Darwin also reports that one species of monkey, the Macacus Maurius, is known to weep copiously. (7, p. 167.)

Effects of Crying. The following examples will show the nature of the facts with which one must deal in treating of the effects of crying:

F., "If it were not for crying I think people would go insane, not having any way to give vent to their feelings."

F., 18. "I feel as though some one had been talking kind to me and a great load had been taken from me."

F., 23. Speaking of despair, "It was always felt before the crying spell, but never after."

F., 19. "After my crying spell I am sure I felt much relieved. The nervousness was gone and I was ready to enjoy life once more."

Among the fifty-seven responses, referring to the adult period all but three express somehow the notion that the effect of crying is good.

The following refers to childhood:

F., 32, a mother. "Unless you can divert them let them cry as much as they can; as a rule they appear to be relieved after crying."

Others refer to the common belief that a baby that cries much is healthy. In seven of the forty-five references to infant cries, the cry is said to be a good means of exercise; that it strengthens the lungs and the vocal cords. The remaining answers refer to the use of crying as expression.

All except two of the twenty-one replies concerning adolescence that refer at all to the expressive significance of the cry also declare the beneficial results of crying. They say "it is good," "a relief," "acts as a safety valve."

The exceptions among these returns emphasize the fact that there is a limit of prolongation or severity beyond which the cry is not beneficial but harmful. One says that although crying relieves the feelings, too much will weaken the power of control. Another, "A limited amount is good." "It is bad for anger, good for grief." "The child should cry when in pain or grief." "The effect is good if the cause is genuine, and the crying is not too hard."

Many other expressions are used to describe the benefits of crying and its limitations.

Seventy-one of the ninety-four descriptions of the cry without objective cause also report the favorable results.

These are typical:

F., 18. "I have an uneasy feeling as though I could not sit down and do anything. After a good cry I feel greatly relieved. It seems as if a great weight had been lifted from me."

F., 21. "I always feel relieved after such a spell, and things generally look bright to me."

F.,— "I have often become so tired, discouraged, or nervous over work and worry that there seemed nothing else to do but have a good cry, and after one has cried a long time relief comes."

F. "A cry sometimes relieves one of that pent up, stuffy feeling and seems to lift a burden that has been resting upon one. Too much repression is bad for the body and the mind both; the mind will not work."

Among the answers to the question in regard to unforced fits of crying, or of "pure misery" in childhood, forty-six of the sixty-one who refer to the effects declare that these were beneficial, giving a sense of easing, refreshment, or relief.

Altogether there is abundant evidence to show that in all of the kinds of crying, except the angry cry, though the immediate physical effect is possibly bad, the mental relief is great, and this seems to be true of all periods of life.

One says, "Although I felt exhausted and sick from crying so much, nevertheless I felt relieved," "I was mentally relieved but physically tired." This state of affairs is repeatedly mentioned in the papers, with a great variety of phrasing.

The termination of the cry by sleep is frequently spoken of especially in the cries of the adult. This is quite what would be expected in the typical condition after the crying spell, physical exhaustion and mental relief. There is relief relative to the previous condition of excitement and a diminished store of nervous energy. Compared with the great distress of the mental strain and unpleasant associations, the physical exhaustion is pleasant; it is like the tired or exhausted state that ensues after a hard day's work, pleasant, if the exhaustion has not been carried too far. It is only when this limit has been passed, and we find the exhaustion greater than the relief, that the experience is predominantly unpleasant. If the physical exhaustion has been carried still further, the sense of strain and effort is not relaxed; the excitement goes on; restoration does not begin and pain is the exclusive result. When over-exertion is local merely, the result is a state in which pleasure and pain are mingled, as in the case of a physical pain such as headache combined with a strong sense of mental and physical relief

All of these characteristics of over-exertion may occur as end phenomena of a crying spell. The predominance of the pleasant or the unpleasant effect seems to be directly related to the severity of the spell. In every instance in which ill effects were reported as the result of crying, the spell has been said to be severe. The conclusion is warranted that the crying spell in itself naturally terminates in a sense of relief, and it is only in cases in which exhaustion occurs that this effect is obscured.

That the ill effects of a prolonged crying spell may be very severe is clearly indicated in the returns. Such expressions occur as "It is a great strain upon the nervous system;" "cried myself sick;" "there was loss of appetite, great physical weakness, and unusual activity of the heart."

Among permanent or long-enduring effects of crying are mentioned: headache, stupor, sickness, exhaustion, nausea, sore eyes.

So far as the references to crying in literature are concerned, it seems that, as compared with laughter with its similar physical reactions, the good effects of crying have been late in becoming recognized. That laughter is good both physically and morally has, according to Sully, become a commonplace to the students of literature. To quote his words "The unlearned, who know nothing of diaphragms, nor of congested veins needing to be relieved, have had a shrewd conviction that laughter sets the current of life moving more briskly. Proverbs such as "laugh and grow fat" attest this common belief.—The learned Burton (b, 1577) quotes a number of physicians in favor of the ancient custom to enliven the feast with mirth and jokes.—Mulcaster, for example (born about 1530), gave a high place to laughter among his physical or health-giving exercise" (44, pp. 34-35). Yet in none of these accounts is the beneficial effect of crying mentioned.

But apparently it is only recently that crying has come to be recommended as a means of physical development. There is now, however, considerable reference in medical literature to the effect that crying is beneficial; that it is an exercise in deep breathing, and tonal expression. Others have recorded its therapeutic uses among the insane for the purpose of relieving mental strain.

Further light upon the question of the relation of crying to emotional tone is afforded by the replies to the questionnaire that have to do with the topic of distraction.

Inquiry was made as to the favorable influence of diversion as a relief from the tension of grief versus the effect of the crying spell; whether the diverting of the attention would relieve the tension permanently or whether there was a tendency for the stress to go on beneath consciousness and break out later. There are about an equal number of affirmative and negative replies on this point. When the cry is voluntarily repressed or attention completely distracted for a time, the tension does not necessarily remain relieved. When the stimulus is intense and the current of thought and feeling too strong for diversion, the relief seems to depend upon the power to cry; if the tension continues, and the victim is carried beyond the point when the natural reaction of the cry is impossible, a state of mind ensues which, in its extreme, becomes entirely abnormal and may go on to insanity. The natural expression of grief is the cry and grief that has passed beyond that stage is essentially abnormal. An instance of the effect of crying upon the state of tension is related in Tennyson's Princess: (45.)

> Home they brought her warrior dead;
>   She nor swooned nor uttered cry.
> All her maidens, watching, said
>   'She must weep or she will die.'
>
> Then they praised him, soft and low,
>   Called him worthy to be loved,
> Truest friend and noblest foe;
>   Yet she neither spoke nor moved.
>
> Stole a maiden from her place,
>   Lightly to the warrior stept
> Took the face-cloth from his face;
>   Yet she neither moved nor wept.
>
> Rose a nurse of ninety years,
>   Set his child upon her knee—
> Like a summer tempest came her tears—
>   'Sweet my child, I live for thee.'

The maidens were wrong in supposing that the outburst could be induced by *increasing* a sense of the bereavement. The experienced nurse knew that it must be diminished. From B. W. Richardson we quote, "As a rule the free escape of tears relieves the heart, and saves the body from the shock of grief. Tears are the natural outlet of emotional tension." Yet he notes that there are exceptions to this rule and "we have more than once seen uncontrollable weeping followed by serious symptomatic disturbances and effects, principally of the heart and circulation; we have known intermittence of the heart beats to be induced in this way, and to assume the most serious character." (37.)

It is said that women who are able to find relief in tears keep their youth longer than those who repress them. Concealment "like a worm in a bud" is not only a beautiful poetic conceit but a profound physical fact. In short, strong emotion should find expression. 'Give sorrow words.'

Dr. S. Weir Mitchell says: "I am very sure that the effect of moral tears on the child or the women in distress is to give a certain amount of mysterious relief, the ultimate cause of which it is rather difficult to determine. This also is quite certain in my experience with people who have suffered great grief and do not cry at any time, that they are either unusual types or fail for want of this secretion to get relief which has been of late obvious in other cases."

As Sully says of laughter that "as a light stimulus to the nerves, it does good by its occasional irruption into a domain which would otherwise have too much drowsy monotony," but that "its benefits are rigorously circumscribed" (44 a, p. 37); so we may say that crying is a similar interruption to overcome a graver condition. The good effects of crying are relative. They are relief from pain rather than a source of exuberance, vitality, and life. After a crying spell life is more often made tolerable than enviable.

Other effects of crying are seen in the cases of people who are normally given to strong self-control. Of the twenty-four answers describing examples of crying in persons usually self-controlled, eleven say that the result is physical exhaustion and prostration. In one case, the illness lasted for a month. Crying spells that occur after long control, as during the illness of friends, are likely to be especially severe and exhausting. One report contains an account of the difference in the effect upon two sisters who had lost their mother. One broke down, at once, and could not control her grief. After the grief had expended itself she rapidly grew cheerful and liked to talk about her mother. The other sister shed no tears, but went about the necessary duties, and only after about a week did her grief find expression. Then she broke down and was ill for several weeks. Another example almost identical in nature is reported.

In regard to such differences, it is likely that in the first case the emotional nature was relatively lighter and the expression of grief less intense. In the second a greater physical vigor withstood for a longer time the outbreak. Where there is already nervous weakness it is likely that the cry point is more readily reached; the accumulation is not so great, and the exhaustion therefore relatively less.

Summary of Part II. Both in its aspects as observed by those who have answered the questionnaire and viewed as a physiological process, crying is a very complex phenomenon. Its description and explanation is necessarily difficult. Still the cry presents itself as a somewhat uniform series of acts, though with very decided changes in form due to age and possibly other conditions.

Briefly stated the essential elements of crying are changes in circulation of a very widespread and general kind, characteristic body attitudes, lump in the throat, vocalization, sobs and tears. The attitude of body varies with the cause of the cry; the typical attitude of the grief cry is one of collapse. The attitudes of grief in ceremonials further illustrate this point and bring out the fact that crying is closely related to actions of abasement and self-torture, renunciation and even suicide.

Vocalization in the cry comes early in the series of acts and it is the first feature of the cry to attract attention in the life of the individual. There is also more vocalization among primitive races than among the civilized. It is from the "cry" as call for help, which is clearly its function in the cry of anger, fear and hunger, that language has in part, at least, developed.

The lump in the throat is a mysterious element. It appears to precede the tears in the cry and to be related closely to the beginning of the sob. Connected both with swallowing and breathing, its function seems to connect it both with the mechanisms of respiration and digestion.

The sob is, for the most part, a late symptom of the cry; it begins, though there are exceptions to this rule, later than the other symptoms and remains after they are suppressed. It is absent in the young child, and there are but few references to it in the ethnological literature. It appears to belong especially to deep grief and, typically, to adult grief. Like the lump, it is connected both with respiration and movements different from those of respiration. It usually comes as a climax to the crying spell.

Tears are absent during the first few weeks of life save in exceptional cases. They are, of all the elements of the cry, the least under control of the will. And, although the evidence on the point is not conclusive, there appears to be more shedding of tears in the young child and among primitive peoples. The cries of anger, fear and hunger of the young child are relatively tearless; it is the grief cry that is essentially the tearful cry.

The effect of crying is declared good, both by the observers who have reported, and by medical opinion. The good effect is felt in a mental rather than a physical relief, since the physical result is commonly exhaustion. Some medical opinion supports the view that crying is a helpful stimulation in the young child and that the cry resulting from grief aids a sluggish circulation and also affords some kind of a relief from a tension or overcharged condition of the nerves.

There are two groups of symptoms or elements in the cry. The first group develops earlier and belongs more especially to the cries of hunger, pain, fear, anger; it has more vocalization and less tears. The deep inspirations and holding of the breath go with this but there appears to be very little or no sobbing. Unfortunately there is little evidence in regard to the facial movements accompanying this cry. All of this complex comprises the cry that is a call for help. It is more active than the second form. The second complex of symptoms comprises the sob, the lump in the throat, the collapsed position, the lack of vocalization and abundant tears. It is passive and goes especially with the hopeless state of mind. The two forms surely overlap in the life of the individual and the symptoms of the two become mixed. The tears and the deep inspiration, especially, appear to accompany both phases of the cry.

These two forms of the cry are not to be thought of as entirely separate but rather as representing, probably, different forms of physical reaction or expression at different moments of a situation, which is characterized by helplessness and a lack of individual adaptation to situations.

It remains now to examine the crying situation more minutely, with special reference to any theories that have been proposed, to explain the movements that occur in it. We shall find ourselves confronted with a mass of interpretation of these acts, which can best be called the physiological or mechanical. Whether the crying acts have any further significance, as coördinated or adaptive acts, can be decided only after a more exhaustive search into the origin and function of the structures that are involved in crying than has yet been made by any one.

III. THEORIES AND INTERPRATATION OF DATA.

This section consists of (I) a résumé of views in regard to crying. (II) A study of crying as a physiological act. The elements of the process as they appear to introspection and observation are each treated under three heads (a) anatomical, including especially a description of the nervous mechanism involved; (b) embryological; the genesis of the structures in the human individual; (c) zoölogical; a study of the development of the structures in the race. With such evidence at hand the theories of crying can more intelligently be estimated. (III) the construction of an interpretation of the crying act as a whole.

(I) *The Theories.* The scattered facts and theories about crying relate for the most part to two aspects of the process. (I) its usefulness, as a large physiological reaction of some kind either to stimulate the circulatory mechanism when it is depleted by grief, to revive the metabolic process, or to relieve the overcharged nervous system. These explanations, which account for the fact that crying has survived, are thought sufficient as a theory. They include references to the good effect of crying in the young child as exercise and to the use of tears or the acts that accompany the shedding of tears. These views can be called physiological or mechanical theories of crying. The second group of suggestions relate to the development and preservation of the crying act as language, *i. e.*, a means of expression of need.

The mechanical theory of crying can be expressed as follows: When in a stage of passive grief the victim is in a state of apathy and collapse, with sluggish circulation and slow vital processes, the depressed state is relieved in one of two ways; the consoling friend is likely to interrupt and urge the sufferer to voluntary exertion and to divert the trend of thought into new channels. If the effort is effective, the fatigued cortical centres obtain rest and nutrition is increased through the improved respiration and circulation that has been brought about by the action. (7, pp. 79-80). But when the sufferer is left to himself, unpleasant trains of associations are followed up, the depressed feeling is increased by them and, in return, reacts upon them, the energy becoming lower and lower until there is an outbreak in a nerve storm of weeping. When the shock of grief is sudden we have the same series of events foreshortened. The natural tendency is to hold the breath; there is a disturbance of the heart and stagnation of the vital functions. The weakness of the knees and the prostrate attitude are signs of the bodily weakness. Now but a small stimulus is sufficient to upset the mental and nervous equilibrium already strained. In either case, in the midst of the voluntary cessation of activity and function, nature takes the case into her own hands and produces the same physiological effect to restore and to re-stimulate the action.

For an explanation of tears we are largely indebted to Darwin whose theory is briefly this. During any violent expiration, as in screaming, the external, the intra-ocular, and the retro-ocular vessels of the eye are affected in two ways (1) by an increased pressure of blood in the arteries, (2) by the return of blood being impeded. It is certain that both the arteries and the veins of the eye are more or less distended during violent expiration. Now nature has taken precaution against injury to the delicate structures of the eye: to prevent the eye becoming gorged with blood, the eyelid is spasmodically contracted and holds it tightly against the cushion of the socket thus preventing an undue expansion of the capillaries of the eyeball and those immediately surrounding it. This movement involves a raising of the inner ends of the eyebrows, which occurs simultaneously with the downward and outward movement of the corners of the mouth, that gives the typical expression of the crying face. (7, p. 147.) "Wrinkled forehead, with eyebrows drawn together, closed eyes, up-drawn nose, and open mouth" is a description of a crying child in our returns. The constant and energetic movements of the face are often seen in the cases of hypochondriacs and melancholics. (7, p. 185.)

Darwin adds that "it is an important fact to be considered in any theory of the secretion of tears, from the mind being affected, that whenever the muscles about the eye are strongly and involuntarily contracted, in order to compress the blood vessels, thus to protect the eyes, tears are secreted often in sufficient abundance to roll down the cheeks. This occurs under the most opposite emotions, and under no emotion at all—the sole exception being in the infant, who does not shed tears until from the second to the fourth month." (7, p. 163.)

The overflow theory of the expression of crying is related to the play theories of Spencer and Schiller (42, 39). This view is, that when there is a condition of highly charged nervous structure or over-nourished cells, discharge in the line of the least resistance tends to take place. Such spreading of energy is made to account for much of the action such as occurs in crying and laughing. Those writers who have been influenced by this theory have, as for example, Sully, been prone to see a very close relationship between laughing and crying. Sully gives examples in which the laugh and the cry occur in connection with the wrong stimulus, *e. g.*, a shock of unpleasant news will produce laughter and the reverse—and this is not a pathological but a very common phenomenon. The laugh that follows after a strain of dignified behavior, the nervous cry after a shock of fear or after close attention or prolonged grief, shows that the laugh is a result of a relaxation from a tense surcharged condition of the nervous system. When the demand for tension is relaxed there is a dispersion of energy and the laugh takes place. The cry is also preceded by a state of tension; and their difference, according to this view, is largely a matter of difference in the manner in which the release takes place. In the case of the laugh there is an interruption; in the case of the cry the tension increases until resistance is overcome.

Close resemblance in the vocalization of the laugh and the cry has been pointed out, and the fact that oftentimes the two are not distinguishable by their vocal expression alone. Text-books of physiology often call attention to the resemblance of the laugh and the cry, pointing out that the chief difference is in the facial expression, and that the physiological events are very similar. Similarity is again attested by the fact that the laugh is often accompanied by a flow of tears.

Such a likeness is very well explained by the overflow theory, for the laugh and the cry are most similar at the point of greatest intensity. According to the laws of spreading of reflex action, when the stimulus is intense enough, structures are brought into play in a regular order and, at sufficient degree of intensity, the physiological expression of emotions are similar. Hence the ease of the transition from the laugh to the cry. As Sully says, "The extremes of boisterous mirth and grief seem to approach each other." Darwin explains the ease of the transition and the close similarity of the laugh and the cry, by the close similarity of the spasmodic movements." (7, p. 208.) "When the motor centres are engaged in the full swing of one mode of action they may readily pass to the other and partially similar action." (44, a, p. 70.)

It is unquestionable that there is truth in the views in regard to the overflow of nervous energy or stimulation, when action is intense. This fact serves to confuse emotional expression, and doubtless crying both as shedding of tears, and as a larger physiological reaction occurs purely as a result of mechanical stimulation, or spreading of effect of stimulation. But it is very doubtful whether this theory will explain all of the crying reaction or the essential differences and likenesses in crying and laughing.

The experiments of Dumas (8) upon the physiological accompaniments of joy and sadness, and his interpretation of them, make an important contribution to the subject of crying.

Dumas found both in joy and in sadness an active and a passive stage, quite distinct in character from each other. In grief the excited or resistant stage is one of over excitation, increased heart-beat and respiration, and vasomotor constrictions leading, on account of the intense effort which it demands of the organism, to a period of fatigue or sadness. The passive stage has opposite physiological characteristics; it is a state of cessation of struggle against the situation. The two states commonly alternate.

The tears, according to Dumas, belong to the excited stage of sorrow in which there is active cardiac excitation and peripheral *vaso-dilatation* and not as Lange maintains to a period of reaction or relief from vasomotor contraction. The tears go with red face, swollen eyelids, etc., and other signs of active secretions, urine, etc., the results of circular hyperactivity and vaso-dilatation.

He agrees with Descartes in saying that extreme sadness is the sadness that yields, the less profound sadness that which resists.

This "resistant" theory of the phenomena of crying is important, and is in accord with the view developed later in this paper, in which attempt is made to interpret biologically the body activities in crying, though it must be noticed that the "resistance" in the active stage is non-adaptive, both in its mental and physical aspects, and needs more explanation than it in itself affords, of the crying situation.

Another view of crying is developed from the nature of the cry as a call or expression of a need. This is best stated by Powell. "Those naturalists who are also psychologists explain the origin of weeping in irritation in which the eyes are subjected to smoke, dust, or other foreign particles, and from scratches and blows. Primitive man seized upon this natural effect of discomfort to artificially produce weeping in order that he might express grief to others (34: CXLII). When used at first it was difficult, but it speedily became easy, and becoming easy it gradually became habitual, and finally instinctive by inheritance. Thus weeping became a linguistic sign . . . like other signs of emotion it may be used in the practice of deception. Similarly the sob has originated in the practice to simulate movements of pain and as in the case of tears, "habit has made it instinctive, but its true nature as an artificial sign is plainly exhibited when sobbing is simulated" (34: CXLI).

Inadequate as this theory is to explain the origin of crying, it serves the purpose of emphasizing the manner in which the cry has been built upon and complicated by being turned in many ways from its course. It is quite true, as Powell says, that "emotional signs are especially characterized by multifarious meanings, for this reason emotional language is highly ambiguous, and a ready tool for deception (34, c). But it is not so certain that the symbolic nature of crying is illustrated by its use in various forms of crying for effect. Crying, however, as the returns to the questionnaire show, does undergo a high development as conscious expression. Many report its use habitually as a tool of deception; it is often accompanied by imaginative dramas of self pity, etc., quite remote from its natural ideationless expression.

Chamberlain has pointed out the significance of the cry in relation to the development of language, and that language has a large root in the cry. There are two language elements, in fact, in the animal cry, (1) the spontaneous reflex cry of emotion, (2) the significance of the interjection, the cry of warning, threat or summons, and the great variety of meaning, which is possible to a single verbal symbol by a mere variation in its tone. By means of stress, reduplication, and intonation, language has been developed (4 b. pp. 118-119). Whatever the natural basis of the cry, it is still more highly conventionalized in the mourning customs of primitive and civilized peoples. Here the form of expression rather than the emotion itself is built upon, and art develops out of sorrow, diffusing the feeling and subordinating it to the consciousness of the expression. Music, too, has one of its roots in the cry, which develops through the rhythmical lamentation and the simple dirge into more complex forms.

Wundt rejects this explanation (*i. e.*, Darwin's) seeing in the lachrymal glands derivative pain assuaging organs. The secretion, which is continuous, cleanses the eye from foreign bodies, such as dust and insects, etc. As the visual images are the most important of all, the shedding of tears would be an unconscious effort to drive away sad representations, having for its foundation an analogy between the painful sensations and the images. (48.)

Ribot comments unfavorably upon this view as well as upon those of other writers. He calls attention to the fact that all attempts to account for tears are based upon their connection with painful states of consciousness. But tears are produced under conditions so varied that this is not adequate. Ribot maintains that the fundamental fact of the physiology of tears is that they are always accompanied by an increase in the circulation, but that this simplicity of mechanism is not incompatible with a diversity of causes. The circulation is accelerated in joy and tears flow as a result of the increased blood pressure. Sorrow is accompanied by a lowering of blood pressure, and in the early stages of crying there is often no shedding of tears. But in the case of sorrow the shedding of tears may well belong to a second stage in which the return of vitality has begun. Now the tears act as a "safety-valve."

That there should be so many divergent and unrelated opinions in the matter by such authorities as Wundt, Ribot, and Darwin indicates, at least, the difficulty of the subject.

Before these theories can be properly judged it is necessary to examine more closely the mechanisms of the act of crying according to the outline proposed above (p. 188).

Physiological and biological data. The lachrymal apparatus consists of the gland, the canals, sac, ducts and nasal duct. Genetically, lachrymal glands first appear in the amniota but the duct first appears in the amphibia. In its embryological development the lachrymal gland arises as a solid growth from the conjunctiva.

Of all the phenomena of crying, the tear is perhaps the most puzzling because it appears under such diverse conditions, physiological and mental. The direct control of the gland is by way of the ophthalmic branch of the fifth nerve, a branch that is also closely connected with the nose, mouth, throat, and respiratory organs. Tears can be directly produced in many ways, such as by stimulation of the conjunctiva, the nasal mucous membrane, the tongue or the anterior part of the mouth; also by powerful stimulation of the retina by light, electrical stimulus, stimulation of the cranial or upper spinal afferent nerves. Venous congestion of the blood will also cause tears. Stimulation of the upper cervical sympathetic is also said to cause a "turbid flow of tears;" but this is a point upon which all are not agreed." (9, p. 1312.)

Darwin's view, supported more or less by the common physiological facts, is that the closing of the eye, or contraction of the orbicular muscle, while causing an undue pressure upon the eye, excites the peripheral nerves, leading to the lachrymal centre. Added stimulation to this centre also occurs as a result of increased circulation of the blood. When the surface of the eye is bruised or irritated tears flow freely. Remote acts, such as coughing, sneezing, laughing, vomiting, all of which involve similar effects upon the circulation and respiration, will produce a flow of tears.

These facts all taken together indicate that no single certain explanation can be made to account for the tear. The mechanisms involved are so interrelated and physiological and mechanical factors so combined that the connection between the tear and a central cause, such as grief, cannot be surely determined. The question is still open, however, whether any mechanical cause alone is sufficient to account for the *large* flow of tears which sometimes occurs in connection with a *very slight* emotional disturbance in which the apparent stimulation of related mechanisms is small and in which there is probably no strong congestion or spreading of stimulus through overstimulation.

The facial movements receive little attention in the returns and the physiology of them can go little beyond referring them to action ot the facial nerve which is concerned in all facial expression, and the further fact that the vagus is also concerned in facial contortions.

The lump in the throat is also obscure as a physiological event. In certain analogous acts there is affection of the throat which may throw light upon the lump. In the globus hystericus the subjective event is that of a constriction of the throat or strangulation. It usually begins with a painful feeling which arises in the lower part of the abdomen, and which mounts upwards until it reaches the neck and then gives a feeling of strangulation. Peterson says that the condition which exists is a "pharyngeal spasm" (5 a). In vomiting, the throat undergoes a change very similar to that described in crying. Here the act is clearly purposive, namely to prevent food from passing backward into the trachea. The larynx rises,—the epiglottis is pulled down to close the entrance to the trachea and the soft palate rises to close the entrance into the nasal passages. The result is a temporary strangulation, respiration stops. A similar act takes place in hiccough, the epiglottis closing the entrance to the trachea.

In connection with the act of crying, attention was called to another phenomenon also referred to the throat, several report as the most important and persistent physical accompaniment of grief or slight effect of a pathetic incident, a pain or discomfort in the throat. This may occur when there is apparently no other sign recognized. Pain in the throat persisted for a year, in the case of a woman who grieved at the death of a child. The suggestion, in a case individually reported, that these are felt to be sometimes an effect of the thyroid, led to a further examination of the point.

There is considerable evidence to show that the thyroid, when enlarged in the condition of goitre, quickly shows the effect of grief and all mental excitement. One case was reported in which the goitre grew largely during any continued mental excitement. Among the chief causes of goitre are mentioned mental and emotional shocks, "especially profound and protracted anxiety and grief." (5, p. 498.)

A brief review of the anatomical facts of the thyroid may throw a little light upon the problem of crying. The organ is highly vascular, its nervous control is by way of the middle and lower ganglia of the cervical sympathetic and through the vagus. Embryologically it is developed in vertebrates as a ventral diverticulum of the mouth or from the pharyngeal epithelium. (38.)

Phylogenetically the ventral portion of the primitive pharynx was concerned in the transmission of food. The special mechanism of this was effected by what was afterward to become the median element in the thyroid. It is suggested that this change of function was correlated with the increasing size of the primitive chordata, and consequently the ability to eat larger prey. The larger food would not have the tendency to escape through the gill slits, and would not need any assistance to pass into the œsophagus. (13, p. 172.) The very close but still imperfectly understood relationship between the thyroid gland and the nutritive processes of the higher animals also adds evidence to its primarily digestive function.

Another effect of crying that is to be referred to glandular changes is the increased salivation; which is, especially in children, sometimes very pronounced, partly accounting for the tendency to swallow repeatedly when crying. The salivary glands are controlled by the facial nerve and also by the sympathetics. The secretion varies with the nature of the nerve stimulation. The saliva obtained by stimulating the chorda tympani nerve is thinner and less viscid than the so-called sympathetic saliva, which is remarkable for its viscidity (9, p. 307).

The sob is a complex muscular act, a combination of a downward pressure of the diaphragm, rhythmically performed, and accompanying actions of the abdominal muscles. It differs from ordinary deep breathing, as one may determine for himself by performing the motion, in that the abdominal movement typical of the sob is in the direction of a constriction and that it is an expiratory movement. The jerkiness is accounted for by the fact that the inspiratory movement thus works against contraction in the direction of an opposition. It may be noted here that the diaphragm belongs to two physiological systems, the digestive and the respiratory, a fact which is illustrated by the part it takes in hiccoughs and in vomiting—acts which need a closer scrutiny with reference to our present topic. There is some doubt whether vomiting is due partially to the contraction of the stomach walls themselves or movements of the diaphragm and abdominal walls only. The event consists of nausea, a reflex flow of saliva and retching movements, which consist principally of spasmodic inspirations with closed glottis. The effect of these movements is to compress the stomach by the descent of the diaphragm and the contraction of the abdominal walls. It is not shown that there are any anti-peristaltic movements. The causes of vomiting are very numerous, including stimulation of the vagus of one sort and another. The afferent nerves concerned in the act are the sensory fibres of the vagus; the afferent are the vagus, the phrenic, and spinal nerves supplying the abdominal muscles. Though the common cause is stimulation of afferent nerves by the contents of the stomach, the vomiting act is very easily produced by remote reflex causes and by emotions—especially fear and grief. The effect of grief upon the digestion may also be mentioned here. (20, p. 326.) The essential point is that vomiting is a "spasmodic contraction of expiratory abdominal muscles and inspiratory diaphragmatic muscles." (20, pp. 325-326.)

Hiccough is caused by a twitch of the diaphragm attended by a sudden closure of the glottis, and is most frequently caused by gastric irritation. It usually results from an excitement of the phrenic nerve; but it is also associated with respiratory centres, a laryngeal spasm and inspiratory movements. It attends certain forms of hysteria. While conversely nausea and hiccough are often characterized by symptoms attending globus hystericus, which is described as a painful feeling arising in the lower parts of the abdomen and developing into the sensation of a round body which mounts upwards—causing a feeling of strangulation in the throat. (5, p. 585.)

In both these acts, the mechanisms both of respiration and the digestive apparatus are brought into play. It must be remembered that the action of the stomach itself as a part of the digestive system is largely autonomous. It is an automatic organ and the stimulation comes from the contents of the stomach. The two vagi and the solar plexus are however connected with the stomach-œsophagus-movements (20, p. 312).

The close resemblance of the sob to these digestive movements cannot be overlooked. The movement of the sob and the vomiting movement are alike in every particular except the apparently accompanying movement of the stomach itself in the movement of vomiting or the internal stimulation from the contents of the stomach. The throat phenomena occur in all of these physiological acts and all are influenced by depressive emotional causes. One-third of the responses to the question whether nausea is an attendant symptom of the cry were affirmative in nature.

All of these movements are results of combinations of mechanisms that belong both to the digestive and respiratory system. The manner in which this close relation has been preserved is easily understood if one recalls the fact that the respiratory mechanism is an outgrowth of the digestive system. Embryologically the lungs develop like a gland out of the œsophagus, at least in all amniotic vertebrates (17). Physiologically the two functions are interacting, both in that they make use of the same motor mechanisms *and in other ways.* The breathing rate is affected by the glosso-pharyngeal nerve, especially in the act of swallowing (11, p. 738). Strong stimulation of the olfactory nerves and fibres of the fifth nerve distributed to the nasal chambers will cause expiration. The glosso-pharyngeal acts with the pneumogastric as the afferent nerves of respiration. Through the pneumogastric nerve various effects become common to the two systems (20, p. 563).

Interpretation of the crying act as a whole. Do such facts as the above afford any means of interpreting the action of crying? On the one hand there is the fact that the vagus nerve controls or may control the greater part of all the actions that occur in crying. The vagus affects the salivary glands and the thyroid, exerts an influence upon the circu lation and causes the globus hystericus as a pharyngeal spasm. It mediates movements and sensations of the œsophagus, movements of the stomach and organs of voice; it tightens the vocal cords, opens and closes the glottis, controls abdominal muscles, the sensations and movements of respiration and the contortions of the face. Two sets of movements, the tear act itself and the facial movements are, in part at least, mediated by other nerves, the movements of the face by the seventh, and the tears by the fifth cranial nerve; the vagus is closely connected with the facial nerve. But the whole nervous connection of these acts is so complicated that no certain interpretation can be made on the grounds of anatomical relationship alone. Even though one should maintain that the whole act of crying is a mere overflow of stimulation and entirely the result of action of the vagus nerve, it must still be asked why of all the combinations that can be affected by the vagus nerve should just this particular set of movements accompany the state of grief.

It remains to ask whether there is any functional connection between the various cry acts. The closest similarity was found between the sobbing act and the act of vomiting; this included similarity of the movements of the diaphragm, the throat phenomena, in part at least and the affection of glands, including tears. That this resemblance is more than a fortuitous one cannot be proved by anatomical evidence alone, and must, in any case, rest upon circumstantial evidence. But there is other proof that this is not a mere accidental resemblance.

If the acts of crying and laughing are considered together it will be noticed that there are here not only phenomena of a peculiar kind, but the antithesis in several important ways of the two will be evident. Obviously the crying act and the laugh accompany typically mutually exclusive and opposite states. The laugh is largely an expiratory act, the cry, that is the typical adult cry, with the sob is an inspiratory act. The muscular movements in the case of the laugh are felt to be of the lower abdominal muscles and are different in character from the sobbing movements, though both are rhythmical. The laughing movement is performed by a bracing of the diaphragm and, at the same time, a rhythmic contraction of the walls of the abdomen so that the diaphragm is gradually forced upward. The movement of the sob, as has been said above, is a forcing downward of the diaphragm with, at the same time, an action of the upper abdominal muscles acting in an upward direction. The facial movements are also opposite. In the laugh the face is as it were pulled together; the corners of the mouth are pulled upward and the eyebrows are brought downward. In the cry the eyebrows are lifted and the mouth is pulled downward. Another well known effect of the laugh upon the lower abdominal mechanisms, namely the relaxation of the sphincters, is to be compared with the effects upon the glandular appendages that occur in crying.

The circulatory differences are not so certainly determined but, in laughter, there is an increased circulation, dilatation of blood vessels, increased muscular movement and the effect upon digestion is beneficial. The opposite conditions accompany grief.

All of these facts put together establish a strong probability of the close connection of the acts of laughing and crying with the movements that are connected with the digestive system—that laughing is the accompaniment of movements that promote digestion and that crying is a part of the process which is involved in the act of rejection of food. This hypothesis receives support from so many sources that it can scarcely be disregarded. The sobbing act itself, with its throat accompaniment similar to the acts of vomiting, the effects of glandular structure are all parts of the digestive movements of one kind or another. The apparent part played by the thyroid is also strongly corroborative of this view. Its close connection with the digestive system phylogenetically suggests that it is directly affected by nervous stimulation. If we accord any credit to the view that emotional expression often involves structures and functions that are at the present time in a state of disuse in the human, the direct effect of the thyroid both as a secretory organ and as a mechanism connected with the movement of food in the alimentary canal would be expected. The further effect of such an action upon the shedding of tears is confirmed by the connection between affections of the thyroid and disturbance of the eye seen in exophthalmic goitre.

Two views have now been exploited one of which explains the act of crying as a whole, as in itself a meaningless physiological act, and the view supported in this paper, which seeks to interpret the series of movements as a connected function. That this latter view is in keeping with the whole tendency of thinking in regard to the expression of the emotions since Darwin's time will probably be admitted by every one. The views in regard to play represented by Groos are in point as contrasted with the older Spencer-Schiller overflow theory, namely that play-actions are co-ordinations of an instinctive character serving ends in the life of the individual.

Many other examples of tendency in the direction of the instinct theory of emotional expression could be given, each one of which strengthens the evidence.

Now, it is necessary to take into account all of the partial conclusions that have been reached in order to see more clearly the place of the crying state among the psychological functions. The physiological events of the cry considered as call in a state of helplessness, as it is seen in the young child (the cry in which the deep inspiration and strong vocalization are the distinguishing features), soon becomes complicated with the phenomena that have been called rejection movements and which are apparently connected with the digestive system. These symptoms, belonging purely to the collapsed stage of the crying state, combine at many points with the call cry. The movements may to a certain extent antagonize each other, as when the effort to call is maintained against the collapse tendency of the nervous system. That the long inspirations of the cry are associated with the effort to cry aloud in order to obtain assistance can hardly be doubted. And there may be in the peculiar inspiratory movements certain useful acts apart from this motive, such as gaining vocal power and at the same time maintaining a firm contraction of the abdominal muscles which is necessary for preparation for strong action. The deep inspiration without this additional effort is one in which the abdominal muscles are relaxed or drawn outward. Similar actions are shown in the practice of girding the abdomen for strong muscular effort, as is sometimes done by athletes.

Thus there are two distinct elements in the physical actions of crying separate in their motive but taking place within the one psychological act followed by collapse or cessation of effort. The call cry with its deep inspiration and strongly active aspect gives way to the hopeless cry which is passive, a breakdown accompanied by body prostration, sobs, tears, lamentations, self-torture, and physiological movements of "rejection." It would be difficult to imagine a combination more clearly representing the giving up of the struggle of adjustment to environment and helplessness. The fear, hunger, and anger cry are all essentially cries for help. The adult does not, as a rule, cry for help, but he helps himself through this stage and then, at the end, breaks down, when the schism between the need and the energy becomes too wide. So that it is quite readily explained why the breakdown form of crying goes only with a high degree of nervous development. Under conditions of tense effort the nervous system repudiates the imposition that is put upon it, breaks through the will and throws to the winds all the accumulation of adaptive machinery and hereditary precautions. The individual throws himself without restraint upon his environment. Like its opposite, laughter, then, crying is essentially non-adaptive in its character. It lacks the incisive character of the *common* instinctive reactions. It is diffused, and unco-ordinated and the movements that are most characteristic of it are not adapted to any immediate end. How shall we interpret such action on the hypothesis of the survival of the fittest among the psychophysical functions. Possibly it can be interpreted as Mosso understands fear, a flaw in the organism. Possibly its usefulness as a restorative of equilibrium in the body or to relieve the nervous tension is quite enough to assure it a place among the functions which have survived. But there is another element, that is, its social significance. The purpose of the cry as call is social; it demands and assumes the co-operation and good will of kind. The hopeless cry also stands for a condition which also calls for some form of relief. The crying act is therefore a psycho-social situation and both the call cry and the cry of despair have come to have a social significance. It is the reverse of the objective side of the act of helpfulness which lies at the bottom of social development. Therefore the ability to cry stands primarily for need, and not for any moral state of the crier. To express his need the infant and the young child are over-expressive. The cry in its effect upon the parent and nurse is an imperative demand quite out of proportion to the actual distress and out of proportion nowadays to the danger of neglect. But under stimulus of instinct the child unconsciously simulates physical pain and severe suffering. The gasp and the prostrate position of the cry of the adult are also extravagant and over-expressive. It simulates or stands for a total giving up and, in itself, in some respects, resembles the actions of dying. The modified cry of civilized man, therefore, stands for this greater event and the elicitation of sympathy, so notoriously accomplished by the mere suffusing of the eyes with tears, is symbolic of the greater need and derived from it. The deep hold that this act has upon the social sympathies is surprising when one understands how much out of proportion to need and even moral deserts it is. Another factor has, of course, assisted in the reduction of the cry and the emphasis of the tear as its symbol, namely, the tendency for all emotional expression to become centered in the face. For with the advent of clothing, the race has come more to "live in its face."

The theory of the cry that is based upon its usefulness physiologically is in the light of this interpretation inadequate ; it reaches to very little depth. Like other actions of an instinctive character crying aims at its mark without special reference to the immediate effect upon the physiological actions of the organisms. Just as we flee in the presence of danger at the risk of our hearts and cerebral arteries, so we cry even at the expense of our nervous energy. The compensation is in the provision that is made for the removal of the particular trouble toward which the instinctive action is directed and the social aid that is commanded. In other regards the body takes a chance. The crier gets his mental relief, and doubtless the body for the most part is not especially benefited by the act.

The acts of crying are, thus, by no means a mere symbol of the act of giving up the struggle. The movements in themselves have significance and are a means towards an end. They are physiologically cessation and even reversal of the will to live and be nourished.

If the question arise as to how these particular movements may have gotten associated with the particular mental states that they accompany, the answer must be because they have never been dissociated from it. Dislike expressed in movements of rejection or withdrawal is the most primitive displeasure: when displeasure is intense it brings to light the movements that are naturally a part of it and which, it is quite possible, are present in the small in all expressions of displeasure. We have learned from recent psychologists to associate non-assimilative states and actions with states of displeasure, and these cry acts are simply these writ large.

If this argument is justified, it is evident that there is in the cry act a very primitive form of emotional expression or physical correlate of the mental event of displeasure. The peculiar non-adaptive nature and diffusive character of the body effects in crying are set apart from the ordinary emotional reactions and we can suppose that it strikes deeper into the organic life than they. In a word, crying is an expression on this interpretation of displeasure in its most generic form.

The social aspect of the cry is so clearly illustrated in the typical early grief cry of the child that its significance can hardly be missed. The child turns his back and covers his face to cry, or throws himself upon the floor, an evidence of the cessation of appeal or effort to obtain something, but social aid is thereby profoundly stimulated and the physiological acts signifying cessation of appeal have come to be more effective than the effort acts themselves.

Summary of the Psychology of Crying. Crying, though an exceedingly interesting and fundamental topic in expression of the emotions, has been a neglected problem. The present approach to it includes the interpretation of from several sources

(1) Returns from a questionnaire, (2) data from ethnological material, (3) physiological data, (4) the scattered literature upon the subject.

Crying occurs under many different mental and physical conditions, but its essential element, psychologically, proves to be a feeling of helplessness in the infant shading into a feeling of hopelessness and surrender of effort in the typical adult cry. The important conclusion is that crying in its last analysis is a situation in which a reaction has taken place at the end of a period of stress in which there has been strong effort and depleted nervous energy. It is essentially a breakdown of the nature of a cessation of adaptation to environmental conditions. An analysis of the crying act itself into its component physiological parts shows the following to be the most important traits: disturbances of the circulation, body attitudes, sobs, tears, lump in the throat and vocalization. There are clearly two groups of symptoms that accompany different moments in the crying act. The first group are the active movements of call, as represented by the vocalization of the young child. The second group, including the facial expression, the sob, the lump in the throat, the tears, prove to be closely associated with movements of the digestive apparatus, and are interpreted as "rejection" movements, going back to the primitive form of rejection of food. The theories that explain crying merely as an "overflow" of energy or as inherited habit seem inadequate. By studying the actions of laughing and crying in relation to each other, still further evidence is obtained for the "rejection" theory.

These movements are, according to this interpretation, a primitive form of expression on the physical side of the mental state of displeasure. The mental and the physical act having never been dissociated from each other, the suggestion is made that, in more subdued form, some such actions occur as the correlate of all states of displeasure. The particular form of expression of helplessness by the cry has been preserved together with its subjective correlate, pity, as a fundamental psycho-social situation.

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WUNDT'S DOCTRINE OF PSYCHICAL ANALYSIS AND THE PSYCHICAL ELEMENTS, AND SOME RECENT CRITICISM.

II. FEELING AND FEELING-ANALYSIS.

By EDMUND H. HOLLANDS.

It is important, in order correctly to interpret Wundt's later theory of feeling, that we clearly understand his position on the subject in his two earliest works. That position may, like the theory of perception which it accompanies, be described as epistemological.

In the *Beiträge*, as has already been explained, the pure sensation is the original element, and the perceptional process is a series of unconscious judgments or inferences.[1] As a result of such unconscious judgment, feeling and sensation in the stricter sense are distinguished as respectively the subjective and objective moments in the pure sensation. This distinction is therefore not original, but the result of reflection. It appears only with self-consciousness and the distinction of the self from its objects. To it no real separation in the pure sensation corresponds, but only a tendency of the stimulus rather to produce an objective impression than a feeling of change in the organ of perception, if the sensation is predominantly objective; or the opposite tendency if it be predominantly subjective.[2] The *Vorlesungen* merely amplify this treatment. Sensations and feelings are both based on an unconscious 'logical' process, and the distinction between them is not a distinction in this process, but in the ends to which they are directed. Feeling is directed to an activity of the subject, sensation to the knowledge of objects. Sensations have the end of knowledge; but we not only know objects, we are also attracted or repelled by them. Feelings are, therefore, an essential part of our physical life, and give to it its color. They all refer to an activity or passivity, to some state, of the self. The distinction between feeling and sensation is contemporary with, and due to, that between subject and object.[3]

Here, then, feelings are distinguished from sensations, as the result of an unconscious judgment, in the process of distinction of subject from object. The *psychological* basis of this distinction is that the feelings are 'subjective' in character, while the sensations are 'objective.' The *physiological* basis is a peculiarity in the process in the end organ which tends to make it an object of attention for itself, in the case of feeling; or, in that of sensation, to cause it to be comparatively unattended to in the interest aroused rather by the 'objective impressions' which it mediates.[1]

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[1] *Beiträge*, 424, 437, etc. [2] *O. c.* 398-400. [3] *Vorl.*, II, 1-5, 15-16.

When we come to the first edition of the *Physiologische Psychologie*, the epistemological distinction has disappeared. Wundt renounces it by name, in a passage which reappears in all the succeeding editions until the fifth. Here it is dropped, probably because Wundt felt that his revision of the *Vorlesungen*, with its explicit disclaimer of his earlier theory of perception, was sufficient. It is important that we see clearly what he means to abandon in this passage, and what not. The sentence reads as follows: "I must, finally, point out, as a conception *partly* belonging to the epistemological view (of feeling), that which I myself once advocated, according to which feeling is everywhere based on an unconscious process of inference, by which the change in our inner state aroused by sensations or ideas is defined as *subjective* (Vorlesungen über die Menschen und Thierseele, Bd. 2.)."[2]

According to this, then, the earlier theory of feeling was only in part epistemological, and that which made it so was its appeal to an unconscious judgment discriminating between subject and object. That this is Wundt's meaning is clear from the fact that he still continues, especially in the second and following editions of the *Physiologische Psychologie*, to distinguish feelings and sensations as 'subjective' and 'objective.' The second edition of the *Vorlesungen*, while its preface definitely disclaims all the matter in the first which it has omitted, reproduces all the passages in it which contain this distinction. It appears then that 'subjective' and 'objective' have for Wundt some psychological meaning. They correspond to some introspective distinction, and what that is we must look to their further application to discover.

We find at once a considerable advance in clearness of treatment in passing to the first edition of the *Grundzüge*. The pure sensation is the primitive element, and in it we distinguish intensity, quality, and feeling-tone.[3] But the feeling is not, like the two attributes, an original and independent constituent of consciousness. In the first place, it is determined by the intensity and quality of the sensation. In the second, the feeling-tone disappears, when we consider the sensation by itself, without reference to the consciousness into which it enters, while from intensity and quality we cannot abstract without destroying the sensation. Feeling is, then, a third aspect superadded upon sensation in so far as it stands in relation to consciousness; or, it *is* this relation. We call it sense-feeling or feeling-tone. All sensations have some degree of feeling-tone, and those in which it is prominent we call (sensory) feelings, while those which have little of it are called sensations in the narrower sense.[1]

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[1] This physiological difference is, of course, not a new theory. *Cf.*, *e. g.*, F. D. E. Schleiermacher, *Psychologie*, 63 ff. (ed. by George, *Sämmt. Werke*, VI.)

[2] *Phys. Psych.*, 460; 2nd ed., I, 496; 3d, I, 540; 4th ('partly' omitted), I, 593.

[3] *Phys. Psych.*, 273.

The second edition points out that, while there can be no feeling without sensation, there must be, because of the indifference-zone, some sensations without feeling.[2] (The first, p. 273, admits this only as a theoretical possibility.) Its important advance, however, is in defining *what* conscious relation constitutes feeling.

Feeling is insufficiently defined by referring it to the general state of consciousness, for sensation "in all its constituents must be conceived as a reaction of our consciousness."[3] Quality and intensity, no less than feeling, can be conceived as subjective reactions of consciousness upon certain forms of outer stimulus. The process underlying feeling is therefore not separable from that underlying sensation. It is nearer the truth to say that in the indivisible whole, a sensation of fixed quality, intensity, and feeling-tone, the latter represents *the constituent in the case of which we are not immediately impelled to refer to objective relations of the stimuli.* In reality sensation, while unanalyzable, is not a simple process either physically or psychically. The *mode of apperception* is an inseparable constituent of all the sensations which we can psychologically examine. Accordingly sense-feeling is immediately comprehensible when we think of it as "*the mode of reaction of the apperceptional activity upon the sense-excitation.*" Hence the influence of the general state of mind upon feeling-tone, and the more subjective significance which we give to it, and also its reference to will.[4]

Briefly, then, the theory of the first period holds that there is but one class of elements, sensations. Feeling is a third attribute of most of these, and represents their subjective aspect. More specifically, it corresponds to or is the mode of reaction of attention upon sensational content. The way in which a perception is taken up into consciousness at large determines feeling. Feeling thus depends on the general subjective disposition in a way which is not true of quality and intensity. Its poles are pleasantness and unpleasantness.

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[1] *O. c.* 426-427, 273.

[2] *O. c.* I, 272, 465.

[3] *O. c.* I, 466.

[4] *O. c.* I, 491-492.

The only material in the second period which concerns our present problem is found in the two articles, *Ueber psychologische Methoden*, and *Zur Lehre vom Willen*, in the Studien of 1883. In the first, we have merely the indication that there is now for Wundt a problem of feeling-analysis.[1] In the second, quality and intensity of feeling are mentioned for the first time, and its close relation to will is emphasized. Pleasure and pain, in the form of desire and repulsion, govern all volition. Feeling is on the one hand inseparable from sensations and ideas, and on the other it could not exist without a will the tendency of which it manifests.[2] These articles, then, show that Wundt was moving toward the position of the third period, where feeling appears as an independent element.[3]

This position is defended for the first time in the *System der Philosophie*, 1889. Inner experience is, we are here told, a manifold of ideational processes, with which feelings are inseparably connected. We directly relate these feelings, as subjective, to the connected ideas, thought of as objective. Since feelings can never be thought of as *objects*, as the ideas can, it is an unavoidable tendency of the naïve consciousness to make them part of the ideas, especially of those which concern the body and its movements, and finally independent ideas themselves. The ambiguous use of the word *Empfindung* has assisted in this perversion of the facts of inner experience. Modern psychology has restricted the word to the elementary unanalyzable constituents of consciousness, and, in a narrower sense, to the elements of ideas. As such, sensations have intensity and quality as attributes. We also speak of the *feeling-tone* of sensations, by which we imply that every sensation has a relation to our own action and passion. This relation, however inseparable from the sensations, is not objectified along with them, nor does it vary in like measure with their intensity or quality. It therefore cannot, with these, be classed as a third fundamental attribute of sensations. Hence the attempt to reduce all feelings and feeling-complexes to feeling-tone of sensations is invalid, and its absurdity becomes evident as soon as an attempt is made to explain in this way the affective result of such a complex as a work of art.

Immediate experience has, therefore, as its content objective ideas and accompanying subjective circumstances. The first task of psychological analysis is, then, to discover the last elements of these two constituents of experience. We may restrict the name *sensations* to the elements of the first, and call the elements of the second *simple feelings*. The separating abstraction is difficult in the case of feeling, because it has not the reference to an object which permits the definite isolation of the idea as an object of attention. Feeling is always attached to some perceptual content, and is particularized only by such a connection. Will, like feeling, always appears in intimate connection with idea and sensation; but the absence of identity between them is established by the fact that the same sensational content may appear without the will, and that a will of the same character may attach itself to an entirely different content.

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[1] *Studien* I, 5-6.

[2] *O. c.* 344, 349-350.

[3] As before, the 3d and 4th eds. of the *Phys. Psych.* reproduce the treatment of the 2nd.

Simple feelings, like elementary sensations, are "primitive facts of consciousness" not to be derived from anything beyond their immediate presence there.[1]

It follows, then, that the content of consciousness divides into acts of will, feeling, and ideation. The three are never separated. Ideation is objective, feeling is a subjective reaction on the ideas, based on the volitional activity of the subject. Pleasure and pain are its chief directions, corresponding to the chief directions of the will.[2]

Wundt's reasons for separating feeling as an independent element, as they appear in the course of his discussion here, may be put under four heads, which have already been mentioned in another connection. (1) Feeling "is not objectified with" sensation. (2) Experience as a whole has an objective and a subjective side; feelings are the elements of the latter, sensations of the former. (3) The feeling attached to a sensation does not vary in like measure with its intensity or its quality, and is not, therefore, like them an attribute of the sensation. (4) While feeling or will is always attached to *some* sensation or ideational content, the connection is not invariable; the same content may appear without the feeling, or a like feeling may attach itself to a different content.

The last two of these reasons, as was pointed out in the previous paper, amount to *independent variability* in the sense in which it was there defined as the test of an element. The first two contain the same 'objective-subjective' distinction which we found used in the first period. As there, so here, Wundt fails to specify the psychological meaning of this distinction.

The chief directions of feeling are still the pleasant and the unpleasant, and as yet there is no mention of the great variety of its qualities as compared with those of sensation. It is immediately referred to the will, but this connection is not new, as the will is with Wundt equivalent to apperception or attention.[1]

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[1] Syst. d. Phil., 380-387.

[2] *O. c.* 562-565.

Concerning the method of feeling-analysis, we find the statement that feeling, since it has no reference to an object, cannot be isolated as an object of attention.[2] It can, however, be particularized by its connection with a sensational substrate. That is to say, this perceptual content can be isolated, and in this way the feeling attached to it may also be separated from the total feeling-content of that moment.

It involves some repetition to reproduce all of Wundt's discussion of the present subject in the article *Zur Lehre von den Gemüthsbewegungen.* But it seems best, at the risk of tedium, to afford the reader every opportunity to examine his terminology in its various contexts.

His primary definition here is, that sensations are the not further analyzable qualitative elements of ideas, in abstraction from the connections in which they always occur, and feeling is the subjective reaction, as pleasure or pain, upon sensation or idea, which expresses our inward relation to the external excitations.[3] The ideas are the objective portion of inner experience. The subjective conditions may be divided into two classes, the feelings and the emotions. The latter cause a noticeable change in the train of ideas. When we abstract in the case of any given emotion from this change, a feeling always remains to which the change was due. The feelings, therefore, must be regarded as the more simple processes. They are essential elements of every emotion, but not every feeling leads to an emotion. The emotion is also connected with concomitant physiological phenomena, which are either absent or very weak in the case of feeling. Feeling is thus the simple element of all emotive processes, as sensation is of all ideational processes,—not further to be defined or analyzed. There is, however, a distinction, in that the conditions of sensation are also simple, while those of feeling are most varied and intricate. Feeling is determined both by the ideational content and by the whole condition of consciousness. It is, therefore, capable of infinite variation, and because of its indefinability we can describe any particular feeling only by a reference to the psychological conditions of its origin and to its effect upon the ideational process.[1]

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[1] *Cf.* Studien, I, 350 and Logik, 2d ed., II, 2, pp. 164-168.

[2] This is the invariable teaching of Wundt. There is, however, a passage in §12, 4a, of the *Grundriss*, which, as translated at p. 162 of the first English edition, seems to contradict it. Here we are told that under certain conditions "the concentration of the attention upon it can generally make any partial feeling whatever predominant." Comparison of the first German edition at pp. 190-191, however, shows that the 'it' of the English version is ambiguous, while the original is quite clear. The attention is directed upon "den eigenen subjectiven Zustand" (*i. e.*, as the context shows, the sensational substrate of the composite feeling), and "in diesem Falle hat dann zugleich *diese* Richtung der Aufmerksamkeit meist die Eigenschaft, ein beliebiges Partialgefühl zum bevorzugten machen zu können."

[3] *Studien*, VI, 337.

Feelings are most intimately associated with the act of apperception. The only justification for our separating them is the fact that they may greatly vary while the effect of apperception upon the ideational content remains the same. The simplicity which we find in feelings, in spite of the complication of their originating conditions, may be referred to the unity of conscious life in the act of apperception.[2]

This article, then, gives us no further explanation of the psychological meaning of objective and subjective. The remark that feeling expresses our inward relation to the external excitation merely points out the problem. All that is new is the insistence upon the infinite variety of feeling, the explanation of this by the fact of its dependence both on perceptual substrate and on total disposition, and the reference of the simplicity of feeling to the unity of apperception.

Neither is anything new to be found in the second edition of the *Vorlesungen*. Its importance in this connection, as we have already remarked, lies in the fact that it reproduces all the 'subjective-objective' passages of the first edition, while decidedly repudiating its epistemologising tendencies.[3] We pass over the fourth edition of the *Physiologische Psychologie* for the same reason.

The article *Ueber psychische Causalität* in the tenth volume of the *Studien* adds nothing to our definitions, but contains a criticism of psychological theories of feeling which seems to show that one meaning in which Wundt uses the term 'subjective' is lack of correlation with any definite physical stimulus. All ideas are more or less intimately connected with *value-determinations*, sensuous, æsthetic, ethical, or intellectual, for which, or for the influence which they exert on the course of mental life, any parallel or analogue on the physical side is lacking. This is because value-predicates are not applicable to physical processes considered apart from the subject. In so far as physical distinctions accompany the psychical here, as in sensuous feelings they demonstrably do, they lack any peculiarity by which one could account for their psychical value. Attempts to explain such feelings by physiological distinctions—for example, the explanation of pleasure as due to the biological value of its stimulus—are only a relegation to the physical side of the value-predicate observed on the psychical.[1] Such an outcome is characteristic of the explanations of materialistic psychology, which all agree in reducing psychical to physiological processes. This reduction may be in the first place to simple sensations, and through them to outer or inner physiological stimuli; or, when the processes are too complicated to admit of this, they may be reduced to *concomitant excitations* (*Miterregungen*) and to physiological nerve-connections as their source. Reflex movement and the accompanying sensation are given as the typical example of such a concomitance. (This seems to be directed against James.) Some psychologists think that the simple sensations suffice, others call in the concomitant excitations. But all materialistic psychologists agree in the tendency to exchange the psychical process for some tangible, but usually hypothetical, physical process.[2]

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[1] *O. c.* 358-360. [2] *O. c.* 365. [3] *Cf. Vorl.*, 15, 224 ff., 238-239.

The third period ends, in the second edition of the *Logik*, by the first explicit discussion of the method of feeling-analysis. As we saw in the previous paper, the *Logik* sets up two criteria for the element, besides the general condition of analysis: that of non-decomposability, and that of possible isolation as an object of attention. This second criterion cannot be met by feelings, for they vanish when the sensation is 'thought away.' They cannot, therefore, be called *independent* elements in the same sense as the sensations. There have been attempts to make them so, either by making them a specific class of sensations, or else by holding that pleasantness and unpleasantness are individual unchanging feeling qualities, instead of mere class-notions. Such attempts simply contradict the plain facts, and the men who assert that these positions are borne out by their introspections do not deserve to be argued with. It is a mere dogmatic prejudice that all subjectively unanalyzable constituents of consciousness must be possible isolated objects of attention. There are simple unanalyzable feelings, but they cannot be isolated in the same way as sensations, because they have no objective reference. The methods of feeling-analysis differ, therefore, from those of sensation-analysis, and are more difficult to apply.[3]

Because of this difficulty, psychologists too often have recourse to unaided introspection in this field, which usually means that they accept such dogmatic prejudices as those of the sensationalistic and dualistic theories, against which Wundt does not deign to argue, at least not here. Such unaided introspection has the further difficulty that it cannot, with certainty, separate all ideational contents from the affective. The only certain method is, therefore, the experimental variation of the conditions, the method of *impression*, the essential feature of it being that it allows the change of only one of the elements in a complex phenomenon at a time. Since the results of the method of *expression* are physiological, it must be classed among the auxiliary physiological methods. However, it has an indirect psychological value, since it aids analysis.

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[1] *Studien*, X, 46.

[2] *O. c.* 57-58, 84.

[3] *Logik*, II, 2 (*der Geisteswissenschaften*), 198-199.

Three special difficulties of feeling-analysis are indicated. Its most important forms can be genetically investigated only in folk-psychology. Its conditions are immensely complicated, and include indirectly, through association, the whole past history of consciousness. Finally, there is an extreme paucity in feeling-nomenclature; the names we have are merely generic, such as pleasantness and unpleasantness.[1]

The third period has, then, thrown some light upon Wundt's theory of feeling-analysis. We cannot, it is true, isolate feelings in the same way as we do sensations; but we can indirectly isolate them *from each other* by the isolation of their perceptual substrate,—given, of course, invariable subjective attitude. This method of indirect isolation, which is pointed out in the *System der Philosophie*, would seem to be the necessary preparation for the feeling-analysis proper described in the *Logik*. The feeling and the perceptual content which arouses it being thus isolated, we discover whether the feeling is simple by varying, one at a time, the elements in that content. If the components in the feeling-reaction vary as a result, or some of them drop out or are added, the feeling is of course not simple. If, however, the feeling is found to disappear or change in its entirety by the alteration of a single element in the perceptual stimulus (once that stimulus has been precisely defined by elimination), then it is simple. This seems to be what Wundt means, and it implies that when the perceptual substrate of a feeling is itself an elementary sensation, then the feeling attached to it is (psychologically) unanalyzable and simple. This question will come up for further discussion later on.

Nothing explicit, however, has been done in the way of giving some psychological meaning to 'subjective.' Wundt has established simple feelings as elements because, like sensations, they can be independently experienced in varying relations, while themselves unanalyzable. What justifies him in making them a *different* class of elements is still their 'subjectivity' as contrasted to sensational 'objectivity.' As we have pointed out, there seems to be some ground to hold that one connotation of the term is lack of correlation with definite physical stimulus. Very analogous to this is the reference, which has been implicit from the first, to total disposition of consciousness as against particular perceptual experience. But as yet we have no clear connection of the term either with these or with any other introspective description.

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[1] *O. c.* 215-223.

This description is supplied in the first edition of the *Grundriss der Psychologie*. The book is important both for this reason and because it gives the first full statement of the final development of Wundt's theory of feeling. It is necessary, therefore, to examine its treatment carefully.

The two classes of elements, sensations and simple feelings, correspond "to the two factors present in immediate experience, the objective contents and the experiencing subject." Both sensations and feelings possess the attributes of quality and intensity.[1] They differ as to certain attributes connected with the immediate relations of sensations to objects, and of feelings to the subject. (1) The qualities of sensation move between maximal differences; those of feeling, between maximal opposites. (2) Simple feelings are much more numerous and various than simple sensations, because (a) "every sensation of the many-dimensional systems belongs at once to several series of feelings," and more especially because (b) simple feelings appear as the subjective complements not only of simple sensations, but also of ideas and of ideational complexes. (3) Sensations fall into separated disparate systems, feelings form a single connected manifold, for every feeling has some qualitative relation to every other feeling. This third difference shows that the origin of feeling is more unitary than that of the sensations. It is the same distinction as that between the unitary subject and the plurality of objects.[2]

This seems explicit. If this third difference is the same distinction as that between subject and objects, then what Wundt means by 'subjective' is, psychologically, the unitariness, the connectedness, of the affective side of experience. That this is the case is, we think, made clear by a passage which immediately follows.

Since, says Wundt, we can isolate sensations from feelings, but never feelings from sensations, we cannot speak of *pure* feelings as we can of pure sensations, and the false view may arise that sensations are the cause of feelings, or that feelings are a particular class of sensations. The first view cannot be held, because feelings always derive from the attitude of the subject, so that the same sensation may be accompanied, under different subjective conditions, by different feelings. The second view is also untenable, because "the two classes of elements are distinguished, on the one hand by the immediate relation of sensations to objects and of feelings to the subject, and on the other by the fact that the former range between maximal differences, the latter between maximal opposites."[1]

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[1] *Outlines* (1st English ed.), 28-30.

[2] *O. c.* 33-35, 81-82.

Now in the passage quoted Wundt obviously uses the subjective-objective criterion as entirely equivalent to the third difference in his list. He is quoting the two differences of feeling as such from sensation as such, and they are given in the first and third of his list of distinctions; the second in the list is merely an external mark derived from the peculiar nature of feeling-conditions, not a specific difference. The first difference in the list he quotes precisely; the other formula may therefore be regarded as equivalent in meaning to the third, and our previous conclusion is made certain.

The 'subjective' nature of feelings, therefore, means their connectedness, their tendency to fuse together; the fact, as Wundt puts it, that they form a *single* system, and that an interconnected manifold, in which every feeling is in some way related to every other, while sensations fall into a number of disparate systems. This agrees perfectly with the hints of Wundt's meaning which we have come upon already. The syncretistic tendencies of feelings, the impossibility of grouping them in definite systems, has as its psychophysical parallel the lack of correlation with definite physical stimulus, and as its psychological the reference to total conscious disposition instead of to particular perceptual experience. It also explains why he has repeatedly said that attention cannot isolate a feeling as its object because feeling 'has no objective reference.' What he meant was, that a feeling is not definitely marked off by its dependence on a particular kind of physiological stimulus, or by its place in a particular modality of experience.

This being granted, then, let us proceed with our review. Wundt has shown that it cannot be maintained, either that sensations are the causes of feelings, or that feelings are themselves sensations. "Because of the objective and subjective factors belonging to all psychical experience, sensations and feelings are to be looked upon as real and equally essential, though everywhere interrelated, elements of psychical phenomena."[2] The other possibility, that feeling might be a third attribute of sensation, is disproved by the fact that the affective tone of a sensation changes in *both* quality and intensity for every change in either quality or intensity of sensation.[3]

Simple feelings attached to simple sensations are easily isolated from their connection by the same method which isolates the sensations; but those belonging to complexes "can never be separated from the feelings which enter into the compound as subjective complements of the sensation." For example, the feeling of harmony connected with a chord can never be dissociated from the feelings attached to the single notes of the chord.[1]

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[1] *O. c.* 37.

[2] *L. c.*

[3] *O. c.* 78.

Sense-feeling cannot exist without the sensation, nor has each given sensation a given feeling. Feeling depends on sensation *and* disposition. There is, therefore, a double abstraction involved in getting at the sense-feeling. The feeling must be distinguished from the concomitant sensation, and also, as unvarying, from all the other feelings connected with that sensation under varying conditions. This second abstraction is very difficult, because the associative factors are always present. Because of this difficulty some psychologists have disbelieved in the existence of any pure affective tone of sensation, and hold that the affection is always due to the associated ideas. But experiments in color sensation show the contrary, for the affective tone of color is greatest when its saturation is strongest, and it is most unlike the colors of familiar natural objects. The uniform choice of particular musical tones to express particular feelings is another disproof of this theory.[2]

Every sensation in a multidimensional system has an associated feeling which is "a resultant of the affective elements due to its position in various dimensions." The discrimination between simple and composite feelings, therefore, cannot be carried out. Both are alike irreducible. The union of some feelings "gives rise to feelings which are not only unitary, but even simple in character."[3]

There are three *chief directions* of feeling in the feeling-manifold: pleasantness-unpleasantness, excitement-depression, strain-relaxation. Any concrete feeling may belong to all three, to two, or to only one of these. Organic sensations, smell, and taste, arouse especially feelings of the first direction; color and clang sensations, those of the second; those of the third are connected with the temporal course of processes, as for example the feelings of expectation and of satisfaction. We may assume that these three directions "depend on the relations in which each single feeling stands to the *whole succession of psychical processes*." A feeling has a place in the first in so far as it represents the state of the present moment; in the second, in so far as it exerts a definite influence on the succeeding state; in the third, in so far as it is determined by the preceding state. Such a connection makes it improbable that other such chief directions exist. These directions also enable us to find in the affective elements the antecedents of the fundamental forms of the emotions. Finally, Wundt concludes his discussion by the remark that the sensationalistic and dualistic theories are beneath criticism.[1]

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[1] *O. c.* 74-75.

[2] *O. c.* 29, 75-77.

[3] *O. c.* 79-82.

This first edition of the *Grundriss* adds much to our knowledge of Wundt's doctrine of feeling. Here for the first time we find the three directions of feeling, and their connection with the temporal course of consciousness. Here, too, is added an important distinguishing character of feeling not noticed before, that of movement between maximal qualitative opposites. Most important of all, what has hitherto been referred to as the *subjective* character of feelings is given a psychological meaning. This is, that feelings form a single interconnected manifold, all feelings are qualitatively related, while sensations fall into disparate systems. Nothing new is said concerning feeling-analysis, but the statement that simple and composite feelings are alike irreducible when they attach to a simple sensation shows that Wundt recognizes the implication in his theory of analysis which we pointed out a few pages back.

The only new thing in the second edition of the *System der Philosophie* is that it relates the three feeling-directions to the temporal course of consciousness in terms of *will*. In the pleasantness-unpleasantness components, a given direction of will is qualitatively specified; in the excitement-inhibition, the degree of energy of will is indicated; in the strain-relaxation, the particular stage of the volitional process is defined.[2]

Such changes as appear in the fourth and fifth editions of the *Grundriss* are all in the direction of increased caution. The statement that "the union of certain feelings gives rise to feelings which are not only unitary, but simple in character," is omitted in the fourth edition.[3] The connection of the three directions of feeling with the three aspects of consciousness has disappeared. This is probably the most important change. The reference to dualistic and sensationalistic theories of feeling as beneath criticism has gone, and in its place appears a brief statement of reasons for rejecting them. They are either entirely inadequate to deal with the problems of complex emotions, or else meet them by substituting reflections for feelings, as in the intellectualistic explanations of æsthetic pleasure. The supposition that the six classes of feeling-qualities, which appear as the poles of the feeling-directions, "are themselves simple, concrete qualities," while it seems to be supported by the introspection of hypnotized subjects (*cf.* Vogt), is contradicted by the feeling-phenomena attached to colors and tones. These differ not only in quantity but also in quality. The quieting effect of sky-blue is not quite that of indigo-blue, the pleasure aroused by the interval of the third is not the same as that aroused by that of the fifth. Nor does it seem possible to account for such differences by assuming the admixture of another feeling. The lack of terms for such finer shades of feeling is no argument against their existence, for such a lack exists also in the case of sensations, though to a much less degree.[1]

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[1] *O. c.* 82-85.

[2] *Syst. d. Phil.*, 373.

[3] *Cf. Grundr.*, 4th ed., 91 with *Outl.*, 1st ed., 82.

In the fifth edition, the phrase "objective contents of experience" is substituted for "objects" in the clause "the immediate relation of sensations to objects and of feelings to the subject."[2] This connection is in line with the psychological definition of this contrast which Wundt has now given. The feelings arising from the fusion of sense-feelings, as the sensations to which they attach are united in ideational complexes, are not called "simple" in character, but "subjectively unanalyzable," "unanalyzable in themselves." The test of invariability under varying conditions, as marking the feeling-tone of simple sensations, is not mentioned. As originally stated, it was plainly at variance with Wundt's whole doctrine of feeling. The statement is also expunged that the feeling attached to a sensation of a multidimensional system, though composite because of its place in the several dimensions, is "as irreducible as a feeling of originally simple nature."[3] One's general impression from these changes is that Wundt is now inclined to believe that the feeling-tone attached to some simple sensations may be analyzable because of the possible independent variation in the components of the sensation. For example, we may vary independently brightness, saturation, and color-tone of a color, and thus perhaps establish a division between constituents of its feeling-tone, if it happens to be really not a unitary fusion.

Full as the treatment of feeling is in the fifth edition of the *Physiologische Psychologie*, it adds little but development and confirmation to what has already been outlined as Wundt's general theory of the feeling-elements. Still, it is necessary to examine it with some care, since it is in all probability his final pronouncement on the subject.

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[1] *Grundr.*, 4th ed., 94-95.

[2] *Cf. Grundr.*, 5th ed., 45, with *Outl.*, 1st ed., 37 and *Grundr.*, 4th ed., 40.

[3] *Cf. Grundr.*, 5th ed., 92, 99 with *Outl.*, 1st ed., 74, 82; 76; 79; and *Grundr.*, 4th ed., 86, 88 (and 91, which prepares the change at 99 of the 5th ed.).

The composite psychical processes fall into objective and subjective classes, Ideas and Emotions.[1] Accordingly, psychological analysis points out two classes of elements: "sensations, the ultimate unanalyzable elements of ideas, which we accordingly may call the *objective* elements of psychic life, and *feelings*, which accompany these objective elements as their subjective complements, not referred to external things, but to the conditions of consciouness itself."[2] That is, "in the inseparable whole which we call a sensation of fixed quality, intensity, and feeling-tone, the latter represents the constituent which we conceive as the *subjective* reaction of consciousness upon our impressions, while we assign to (the impression) the intensity and quality of the sensation as its objective attributes."[3] Simple sensations and feelings are then the *irreducible* elements of experience. The task of psychology is not to attempt to derive them from some common middle thing which we never experience, but to show their general relations. They appear as the complementary elements of psychic life, representing its objective and subjective sides, since it is made up of the "experiences of experiencing subjects." "Feeling-tone is accordingly in so far a necessary complement of sensation, as each sensation belongs to a sensing (*empfindend*) subject." It stands as a subjective factor beside the objective, to express the relation of the subject to the impression. This difference shows itself immediately in the fact that we objectify our sensations but not our feelings, which we conceive rather as our own subjective experiences, the reactions of our consciousness upon the objective impressions given in sensations.[4]

These passages have been purposely given *in extenso*. They certainly, at first glance, seem very epistemological in character, but we must remember that if our previous understanding of him was justified, all Wundt can mean by this distinction of objective and subjective is just the difference between those parts of experience which we can definitely particularize in attention, and those which, whether because of their functional interdependence with the attentional process or for some other reason, we cannot so separate. If we look at the actual differences between sensation and feeling, which the *Psychologie* in various passages indicates, this is, it seems, what they all centre in, here as elsewhere.

In the first place, then, the physical conditions of sensation are for the most part external and obvious in the organism, while those of feeling are hidden and complicated.[5] But more especially, the psychical attributes of sensation are much simpler, being only, in general, intensity and quality, while feeling has various additional characteristics. (1) *Feelings move in contrasts* (*i. e.*, between opposites).[1] (2) They form a single interconnected manifold. There is a *general relationship* of feelings which are referable to wholly heterogeneous objective sensations. *All* sensations may arouse pleasantness or unpleasantness, may excite or depress us. Hence the feelings are the chief source of the connections and relations in our mental experience.[2] (3) The feelings present at any moment always tend to fuse into a *total* feeling expressing the collective feeling-content of consciousness at that moment. For this reason the simple feelings are difficult to fix upon, for they fuse into partial feelings, and partial feelings fuse into total.[3] This tendency of feelings to fuse, and the connectedness of the feeling-continuum in general, is to be explained by the fact that every feeling is "a reaction of apperception upon the single conscious experience;" for the unity of consciousness is due to apperception.[4] (4) Finally, and as a result of all these characteristics, feelings cannot be attended to for themselves as sensations can.[5]

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[1] *Phys. Psych.*, (^{5}) I, 344.

[2] *O. c.* I, 14, 350.

[3] *O. c.* I, 354.

[4] *O. c.* II, 356.

[5] *O. c.* I, 353.

Now these characteristics of feeling, taken together, only recapitulate those marks of its subjective character which we have found indicated elsewhere. That is to say, feeling is not correlated with definite physical stimulus, it is to be referred to the totality of consciousness, it refuses to be grouped into definitely separate forms, and, finally, as the result and summing-up of all these qualities, it is always falling into unitary masses, it forms a single continuum. This, then, we may take as Wundt's final meaning in psychology for *subjective*.

The refusal of the sensationalists to accept this separation between feelings and sensations Wundt meets by the assertion that their doctrine is not really psychological, but due on the one hand to the influence of Herbartian intellectualism, and on the other to that of the physiology of sense-perception. The psychologists who hold it demand that there shall be only *one* kind of element, that produced by the stimulation of definite sensory nerves or of their end-organs. This demand is stated as a necessary dogmatic presupposition; it is also stated as a result of immediate introspection (Ziehen, Münsterberg). But the dogma is not self-evident. If anything, the position is *à priori* improbable, for it is natural to look for different elements on which the distinction of subject-object which runs all through our mental life may be founded. The real source of the dogma is not, then, *à priori* probability, but the long-rejected sensualistic theory of knowledge. As for the asserted introspective evidence, it is not found by the unbiassed observer. Empirically, such an hypothesis can account neither for the connection of like feelings with entirely disparate classes of sensations, nor for the connection with complex processes of feelings which are the same in type as those accompanying simple sensations. To explain the unity of feeling, it has recourse to a hypothetical 'common-sense.'[1]

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[1] *L. c.* [2] *O. c.* I, 353; II, 308. [3] *O. c.* II, 308, 341.

[4] *O. c.* II, 341, 357. We may add in passing that we are unable to find in these passages, which are those cited by Dr. Washburn, *Phil. Rev.*, Jan., 1905, p. 21, any justification for her statement there that Wundt refers the unity of feeling to the *physiological* unity of the apperception-centre. At II, 360-362, he hazards the surmise that the connectedness of the expressive movements of affection, vascular and otherwise, may be due to such a centre.

[5] *O. c.* I, 352; II, 305, 308.

The *Psychologie* adds nothing to the theory of feeling-qualities beyond a more explicit statement of the general position. Feelings have quality and intensity, and, besides these, other attributes due to their position in the unitary feeling-continuum, which is, as before, tri-dimensional. That is, their quality is a function of their place in these three dimensions, which we may accordingly call the *components* of feeling-quality. These components are not real feelings any more than the components of a motion are real motions, or than color-tone, brightness and saturation of a color-sensation are real sensations. Immediate introspection shows that they are "basal forms, each one of which includes a number of feeling-elements, but not concrete particular feelings, which reappear in unchanged quality in each single case to which we apply the names given to them." Thus, for example, the pleasure of the taste of sugar and that of the smell of menthol are alike in *direction*, but they are not at all *similar*.[2] The feeling-tone of a sensation depends for "its peculiar quality, specific for each sensation," upon the quality of that sensation.[3]

Nothing new is said concerning the criteria of feeling analysis, but it is important to note carefully what little is mentioned. Its methods are those of impression and expression, and in the former the sensation which is the *direct* result of the stimulus is used only as a means to arouse the feeling.[4] Simple feelings are like simple sensations in the possibility of separating them from their connection by analysis, and in the fact that in this process they become in a sense abstractions.[5] But while sensations can be abstracted from feelings, feelings cannot be abstracted from sensations.[6] Of the two conditions for analysis which are mentioned in this work, therefore, 'independent variability' and the isolating effect of attention, the latter is not mentioned in connection with feeling analysis. This has already been emphasized in a different connection in the preceding paper. Simple feelings Wundt defines as those which cannot be separated into simple feelings appearing independently for themselves.[1] This definition connects itself quite clearly both with the general method of analysis and with the method of feeling-analysis, as it has been already described. The simple feeling is one which can appear for itself in changing relations to other mental content, but which cannot itself be divided by division of its sensational substrate, or, under the further refinement introduced in the *Grundriss*, by variation of the components of that substrate, if it happens to be a sensation belonging to one of the multidimensional systems. Finally, it is essential to remember that Wundt has already stated that feeling-tone is "specific for each sensation."[2]

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[1] *O. c.* I, 352, 359-360.

[2] *O. c.* II, 305-308.

[3] *O. c.* II, 318.

[4] *O. c.* II, 263.

[5] *O. c.* I, 308.

[6] *O. c.* I, 352.

To sum up, then, we find that in this last work the objective-subjective distinction between sensation and feeling is strongly emphasized. But it is given introspective content. The unitariness of the feeling-continuum is dwelt upon, as it was in the *Grundriss;* and to the first and third of the special characteristics of feeling there pointed out, there is added a further result of this unitariness, the tendency of feelings to fuse into a mass or total-feeling. This unity of feeling is referred to the unity of apperception; that is, to the unity of mental processes as a whole at any given moment.

The quality of a feeling is made a function of the three 'dimensions' of the feeling-manifold as components. Wundt's statement of pluralism remains as dogmatic as before. He has no hesitation in appealing in justification of his own position to that 'immediate self-observation' which he justly censures when employed by his opponents. But his explanation of the three 'directions' by connecting them with the three temporal aspects of mental process has disappeared, and in its stead he seems inclined to find an analogy in the three dimensions ot the color-system.

In concluding, it does not seem necessary to restate what, as it seems to us, we must hold that Wundt means by the terms subjective and objective as applied to feelings and sensations respectively. His meaning is psychological, and he is not using an epistemological distinction, as Dr. Washburn supposes. Indeed, his meaning for 'subjective' includes as one of its elements just that lack of localization which the critic herself admits as part of its psychological meaning.[3] With her conclusion that only the pleasantness-unpleasantness feelings fully satisfy the psychological definition of subjective, either in her own sense or in that broader one which we have outlined, we are inclined to agree; but that is a matter which does not concern us here.

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[1] *O. c.* II, 305. [2] *Cf. ante.* [3] *Cf. Phil. Rev.*, Jan., 1905, pp. 26 ff.

Nor does it come within the province of this paper to discuss the validity of Wundt's grounds for separating feelings from sensations. But it is not, perhaps, over-captious to ask why the critic seems to slight the first of the three distinctions given in the *Grundriss*, that of movement between qualitative opposites, when pointing out the insufficiency of the second, upon which Wundt seems to lay no special stress, and which does not appear in the *Physiologische Psychologie*. Given Wundt's general position, the fact that he finds a certain class of conscious phenomena ranging in quality between conscious opposites, and falling into a unitary interconnectedness which can, as it were, indifferently include all the disparate systems of other elemental conscious phenomena, seems no insufficient reason for regarding that class as different in kind from these other phenomena. It must also be remembered that we have found his actual reasons for the distinction to be more inclusive than his explicit statement of them.

As to Wundt's theory of feeling-analysis, it seems to us that such a review as that just completed is almost indispensable to make one's view of it clear. As we have found, it depends on the method of impression and on that of expression. The latter method is, however, merely an auxiliary guide in the actual task of analysis. While we cannot isolate a feeling as an object of attention, yet we can isolate it mediately by the isolation of its perceptual substrate. Once isolated in this way, we attack it in the method of impression by varying, one at a time, the elements in that substrate. If in this way we can eliminate some of the aspects or constituents of the feeling, while retaining others, it is complex; but if we can make no such breach in it, it is—subjectively at least—unanalyzable. If, finally, we have come upon a feeling which has an elementary sensation as its basis, that feeling is, *ipso facto* and by the terms of our analysis, psychologically unanalyzable; for we can introduce no variation within its sensational stimulus. The only exception to this rule is in the case of the sensations belonging to multidimensional systems, such as light, where the possible variation of the components of the given sensation may establish a division in the feeling-tone.

Looking at the criticisms of Dr. Washburn's second article with these facts in mind, we see that Wundt would accept none of the three methods for establishing the simplicity of a feeling which she suggests as the only ones possible on his definition. The first two, that of appeal to introspection and that of appeal to the uniqueness of the sensational substrate, are incompatible with his whole line of argument, as she herself points out. The third suggestion is, that we ground our distinction of the simple feelings from the complex "on our knowledge that the sensational source of the so-called simple feelings is simple, while the so-called complex feeling is derived from a complex sensational source."[1] This contains an element of Wundt's method, but a very partial element. Complexity of sensational source would not determine complexity of feeling for him. That would be a point to be settled by experimental variation in the elements of the complex sensational stimulus. On the other hand, when we have a sensational substrate not only simple, but also such that we cannot even vary its components —a sensation belonging to a one-dimensional system, for example—then we may be sure that the feeling attached to it is psychologically simple. For, as we have seen, the feeling of each sensation is specific for that sensation, and the invariable stimulus must produce invariable feeling-reaction. The feeling cannot, therefore, be duplicated elsewhere in experience, and no division can be introduced into it; and such a feeling is, for Wundt, simple.

It may be well, in closing, to sum up categorically the points on which we have found it necessary to take issue with the critic both here and in the preceding paper. Dr. Washburn's criticisms were: (1) It is not clear whether Wundt's criterion of analysis, independent *variability*, includes independent *existence*. (2) His criterion for the attributes, independence of the mental context, is insufficient to rule out clearness as an attribute. (3) His distinction between feelings as subjective and sensations as objective is epistemological and extra-psychological. (4) His reference of the unity of feeling to that of apperception, and his consequent definition of the simple feeling, make it impossible to distinguish between simple and complex feelings, save by reference to their sensational substrate.

But we have found that, when the whole development of Wundt's doctrine is examined, and the actual way in which he goes to work at his problem, none of these criticisms is valid. More specifically we find that (1) what marks off the element in Wundt's eyes *is* its separability, the fact, that is, that while no breach can be made in it, yet it can be experienced in different mental contexts. (2) Wundt has *four* criteria for the attributes, instead of merely one. These are structural necessity, independent variability, inseparable connection, and independence of the complex. He regards clearness as excluded by the last two of these. (3) The difference between feelings and sensations, as subjective and objective, is not for Wundt epistemological, as he has explicitly disclaimed his own earlier theory of that nature, but psychological; and he has given it introspective definition as a contrast between tendency to fusion and persistent discreteness. (4) Wundt's canon of analysis is the same for feelings as for sensations, though his method is necessarily different; and the reference to sensational substrate involved is never regarded as directly settling the question of the simplicity or compositeness of the feeling, which is on the contrary determined by experimental variation of that substrate considered as the feeling-stimulus.

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[1] *Phil. Rev.*, *ut ante*, p. 26.

Finally, it must be admitted that these criticisms are of real service in pointing out inadequacies in the immediate statements of Wundt on these matters. We have been able to find no single discussion of them in his works which was entirely clear and sufficient by itself. Such criticisms of method and definition are always useful in the case of authors who, like Wundt, have been constantly busied in a varied literary activity, and that in a science which has been too much occupied with growth to examine its presuppositions. They force his students at least to attempt to get a more precise idea of his meaning; and sometimes they elicit a final attempt at definition from the master himself. Such a result in this case would be most desirable.

PERIPHERAL AND CENTRAL FACTORS IN MEMORY IMAGES OF VISUAL FORM AND COLOR.

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The observations recorded below were made in the Cornell laboratory in 1904-5. The general object of investigation was the appearance, recurrence, and duration, in immediate memory, of visual images of simple figures. The purpose of the study was to ascertain whether figures viewed under uniform conditions of attention differ in reproductivity; and, if so, whether or not these differences can be correlated with certain intrinsic characteristics of the figures, such as size, brightness, color, outline, complexity of contour.[1]

It is a familiar fact in everyday experience that, under favorable conditions, memorial images of objects recently experienced tend to recur spontaneously. This fact the experiment sought to turn to account in attacking the problem of the intrinsic liability to recurrence and persistence of images of different figures. The study of the spontaneous recurrence of images was chosen, and voluntary retention expressly forbidden. This procedure, it was believed, promised most toward the elimination of chance reinforcements through associations and purely subjective factors, and was hence more likely to lay bare any relation subsisting between the reproductivity of the figure and size, brightness, etc.

In the first group of observations, the individual durations and the number of appearances of memory images of different figures were recorded. It soon became evident, however, that in the procedure followed the conditions of reproduction were complicated by such factors as affective tone, contrast, familiarity, and associations; and that distinctness and vividness were more constant and more significant factors than mere duration or recurrence. In later groups, accordingly, precautions were taken, especially in the ordering of the series, with a view to further elimination of these disturbances; and a record also of the distinctness and completeness of images was kept.

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[1] This problem, along with the involuntary procedure in the study of images, was suggested by an article by F. Meakin: Mutual Inhibition of Memory Images, *Harvard Psych. Studies*, Vol. I, 1903, 235. In this article, which deals with data relating only to simultaneous observations on a double set of images, the author generalizes with regard to the conditions of reproduction of a *single* image, and concludes that the persistence of an image depends on the activity of its motor accompaniments, and that these motor accompaniments are conditioned by the size, brightness, etc., of the figure. (See I. M. Bentley's critique, *Phil. Rev.*, Vol. XIV, 1905, 253). The method here followed differs from that of Meakin in utilizing one, instead of two figures at a time, thus securing uniform and undivided attention for each; and in recording the duration of each image with greater accuracy.

The second group of experiments was arranged to investigate the relation obtaining between the motor conditions excited by a figure, and the distinctness and duration of the corresponding visual image. The special object here was to verify the conclusions of Meakin[1] and Slaughter[2] as to the dependence of the stability and clearness of the image upon ideated or realized eye-movements. In the third group both voluntary and involuntary recall were studied with a view to discovering whether constant differences in distinctness, completeness and duration hold only for passive attention or run parallel in voluntary and in involuntary recall; and, secondly, whether these differences are correlated with the incentives to sensory attention afforded by the figure.

GROUP I. INVOLUNTARY METHOD WITH FIXATION.

MATERIALS.

Geometric figures cut from colored or neutral-tinted papers were used. These were arranged in five series of four members each, each series being designed to test the dependence of the memory image on a certain factor. The varying factors were outline, complexity of contour, size, color and brightness. The series were made up as as follows:

Ser. I. 1) ellipse, 2) triangle, 3) disc, 4) square—each of medium gray, with a surface of about 12 sq. cm.

Ser. II. A duplication of Ser. I with notched instead of plain line margins.

Ser. III. Discs of the standard gray, of graded sizes, in the ratios 1:2:4:8 and 3-7 cm. in diameter, the next to the smallest identical with the disc of Ser. I.

Ser. IV. Discs of the standard size of Ser. I, of white, light gray, medium gray (the standard gray of Ser. I-III), and black.

Ser. V. Discs of the standard size of Ser. I in saturated red, green, blue, and yellow.

These series were given in the order IV, II, V, I, III. The sequence within the series was haphazard, but constant throughout the whole experiment, being merely reversed every other observation period.

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[1] *Op. cit.*

[2] A Preliminary Study of the Behavior of Mental Images. *Am. J. Psych.*, XIII, 1902, 526.

METHOD.

The recurrence and duration of the memory images were recorded on a revolving drum by means of a key operated by the observer. The observer sat before a table on which, at a constant distance of 65 cm., stood a large black felt screen. Each figure was exposed in turn at the centre of this screen for 4 sec., during which period the observer steadily fixated a point at the centre of the figure. At the end of the fixation period he received a signal to close his eyes. After a pause of 15 to 20 sec., to permit the fading out of after images, he was given a ready signal, and immediately placed his finger on the key preparatory to recording the course of the memory image. The instructions to the observer were to await as passively as possible the entrance to consciousness of the visual image of the figures, indicating its appearance by a light pressure, its disappearance by release of the key. At the end of the record minute, the observer reported briefly on the completeness and distinctness of the image. On the completion of the series, a general introspective account of the course of the image, associations, direction of the attention, variations in the disposition of the observer, was given. The observations were repeated at intervals of one week, in reverse order. The practice observations are excluded from the averages (Table I), which represent five observations for each figure. A special set was also given in which a full introspective report was taken immediately after each record minute.

The observers[1] represent two rather different types, P. being a good visualizer, while C. was predisposed to motor imagery. At the start, both encountered some difficulty in assuming the semi-passive attitude demanded by the experiment, and in attending simultaneously to the elusive flittings of the memory image and to the manipulation of the key. After a moderate amount of practice, however, the temptation, to voluntary recall or filling out of the image was less insistent and the observer's response to the appearance and disappearance of the image through the pressure and release of the key became fairly automatic. A tendency to the formation of an artificial rhythm was anticipated, but seems to have played a very inconsiderable part.

I. *Introspections.* 1. Manner of appearance of image. As a rule, the memory image appears spontaneously at the beginning of the recording period, or in the preceding after

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[1] The writer wishes to acknowledge her indebtedness to the two observers, Miss Peirson and Mr. Coffin; and to Professor I. M. Bentley, under whose direction the investigation was undertaken and completed.

TABLE I.

	Obs. C.							Obs. P.						
	(1) Tot. dur.	(2) Av. dur.	(3) M. V.	(4) Av. No.	(5) M. V.	(6) Tot. dur. for series.	(7) Av. dur. for series.	(1) Tot. dur.	(2) Av. dur.	(3) M. V.	(4) Av. No.	(5) M. V.	(6) Tot. dur. for series.	(7) Av. dur. for series.
Ser. I, Disc	65.7	2.2	.4	6	.8	257.5	2.2	38.6	1.1	.5	6.8	2.5	219.2	1.4
Ellipse	61.5	2.2	.3	5.4	.8			50.1	1.3	.5	7.2	3.4		
Square	61.4	2.2	.3	5.4	.4			76.1	1.8	.7	8.2	1.1		
Triangle	68.9	2.3	.4	5.8	.3			54.4	1.4	.8	7.6	1.7		
Ser. II. Disc	55.7	2.1	.4	5.2	.6	265	2.3	35	1.6	.3	4.2	3.8	236.9	1.6
Ellipse	66.9	2.5	.7	5.2	.9			75.6	1.8	.8	8.2	1.9		
Square	75.4	2.7	.6	5.6	.9			74.1	1.9	.9	7.6	3.6		
Triangle	67	2.2	.4	6	.4			57.2	1.3	.6	8.2	1.8		
Ser. III. Size 1	56.6	2.4	.5	4.6	.7	249	2.4	62.2	1.6	.8	7.6	1.6	228.7	1.8
" 2	66	2.3	.5	5.6	1.2			37	1.1	.6	6.6	.8		
" 3	56.8	2.4	.6	4.6	1.1			37.1	1.2	.7	5.8	1.8		
" 4	69.6	2.4	.5	5.8	1			92.4	3.5	2.5	5.2	1		
Ser. IV. Red	63.3	2.1	.4	5.8	.9	296.7	2.3	64.1	1.7	.9	7.2	1.5	271.3	1.8
Blue	81.7	2.5	.7	6.4	.8			58.6	1.3	.5	8.4	2		
Green	90.9	2.6	.5	6.8	.6			70.3	2.1	1.3	6.6	1.5		
Yellow	60.8	2.1	.6	5.6	1.1			78.3	2.2	1.1	7	1.2		
Ser. V. White	80	2.4	.5	6.6	1.4	296.7	2.3	126.4	3.5	1.9	7.2	2.6	344	2.2
Lt. gray	82	2.4	.5	6.8	1.3			67.3	1.9	.9	7	1.2		
Med. gray	68.9	2.3	.5	5.8	1.2			47.3	1.6	.5	5.8	2.9		
Black	65.8	2.2	.4	5.8	.7			103	2	1.7	6.8	2.9		
	Gen. av. 2.3							Gen. av. 1.7						

In the above Table, column 1 gives the total duration of memory images in five observations.

2 and 3, the average duration of single image, and mean variation.

4 and 5, average number of images per minute and m. v.

6, total duration of image for series (five observations).

7, average duration of single image for series.

image period. Thereafter it returns at irregular intervals, which usually grow longer toward the end of the minute. On a few occasions, C. reports, the image was apparently evoked by chance twitches of the eyeball or eyelid, by inspiration, or, automatically, by rhythmic pressure of the key. Occasionally, also, the observer reports a faint anxiety at the momentary failure of the image, and a temptation to summon it by movement of the eyes (*O.* C.), by steady fixation, or by recall of detail after detail (*O.* P.).

2. Localization of image. The memory image usually appears in the same direction and at the same distance as did the original. P. distinguishes it from the sensory after image by its position outwards on the screen (the after image appearing

"on the eyelids"), and remarks that "its appearance is often accompanied by the feeling of turning toward it." Occasionally it seems to be situated "in the head," but in this case its distinctness is materially lessened.

That this localization is correlated with the presence of motor elements, actual or ideated, has abundant evidence. Thus C., noting that the memory image usually appears as an object with spatial relations, states that in this case "the feeling of accommodation" is present, with the "tendency to move the eyes and locate the image directly in space." The less real this feeling (of accommodation and convergence?), the less distinct the image. Thus, toward the end of the recording period (C. sometimes reports), the images become less vividly 'visual,' are accompanied by almost no tendency to fixation, and are localized, not in any definite portion of the visual field, but vaguely, 'in the head,'—a type of image described by C. as 'more subjective,' or 'more purely memorial.'[1]

It seems probable that P. also refers to the muscular sensations attending fixation in her less concretely phrased account of the semi-spontaneous recall of images. "I seem to turn my attention toward the place where I expect the image to appear. If I hold my attention on this place, several more images are likely to follow." And again, "my attention vacillates about the place on the board where the image is expected, then settles down, and below unfolds the image, sometimes indistinctly, but as the attention turns more decidedly toward it growing in vividness."

3. Incompleteness of image. Images are rarely complete. The lower right hand portion is most often missing, and the upper left hand portion the most distinct,—a condition possibly correlated with the characteristic grouping of matter on the printed or written page, and the acquired habit of attending primarily to the upper left hand word. In cases where the outline is complete, it is often doubtful whether there are not gaps in the main body of the figure. Whether complete or incomplete in relation to the original, the image is usually reported as flashing in and out as a whole, without growth or alteration.

II. *Duration and Frequency*. 1. General uniformities and variations. Two points are first to be noted, the wide fluctuation in individual values for the same figure, and the smallness of the differences between the averages for different figures. The durations for the figures showing the greatest variation swing between the limits 16.8 sec. and 0.4 sec.; and, in a typical case, a mean variation of 0.6 sec. stands with an average of 2.7 sec. (*O.* C.), and a m. v. of 0.9 with an average of 1.9 (see Table I, columns 2 and 3). A glance at the Table will show that, in comparison with these mean variations, the differences between averages for the single figure (column 2), or between averages for the series (column 7), are too slight to be used incautiously as the basis of generalization. It may here be noted that the magnitude of the mean variation is due in part to a progressive shortening of durations which occurred in the course of experimentation, parallel with the decreasing novelty of the stimulus figures; in part to associations, minor distractions, and the like, which often came to light in the introspective notes.

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[1] C.'s feeling of unreality with regard to reproductions of purely retinal elements is interesting in view of his already mentioned 'motor-mindedness.'

The difference between the records of the two observers is considerable. For P. the images are more frequent and of shorter average duration, the mean variations larger, and the differences both between individual averages and between averages for the series greater. This greater range of variation is possibly correlated with the fact that P.'s general practice in observation was less extended, her attitude toward the experiment less stereotyped and more subject to disturbance than C.'s.

In spite of differences, however, the total durations for the two observers are not widely at variance, and the general rise and fall in values from series to series runs fairly parallel.

2. Duration and outline, complexity, etc. a. Outline. (Ser. I.) For duration of images, angular outlines have a slight advantage over curved (especially for P.). This is no evidence of any intrinsic difference in reproducibility of curves and angles, since both recur unsolicited. It seems rather to be a matter of the absence or presence of a number of definite points to attract and maintain attention during the reproduction period. Apropos of this, the observer remarks that in memory the margin and angles of the square and triangle stand out most distinctly, while in the disc the central portion is equally vivid with the margin. It seems, then, to be a matter of diversity against monotony in maintaining the attention.

b. Complexity. (Ser. II.) The duration averages for notched outlines stand in general above those for plain line contours. This does not, however, indicate any correlation between duration and extent of outline, for the observers report no tendency to follow the outline of the image with the eye. They do report a feeling of effort in the presence of the figures of Ser. II, a difficulty in grasping the whole, which is apparently the correlate of the attraction of the line of regard simultaneously to a large number of similar points, *i. e.*, the notches on the margin. Hence, as with the angular figures of Ser. I, the factor determining duration would seem to be the incentives to involuntary attention offered by the image during the recording period.

c. Size. (Ser. III.) Increase in size is not paralleled by increase in duration of memory image. The one exception, in the case of size 4 (*O.* P.), is apparently due entirely to reinforcement through verbal associations, 'large' or 'larger,' in some cases, in others to direct comparison through recall of images of smaller size. This complication was apparently accidental, due to a faulty arrangement by which the series emphasized contrasts.

d. Color. (Ser. IV.) For both observers the general average for the color series is higher than that for the plain gray disc —, 2.3 sec. as against 2.2 sec. for C., 1.8 sec. against 1.1 sec. for P. This argues little, however, for the intrinsic reproductivity of color, since the single averages for two of the colors fall below the standard gray disc averages, while the lengthened duration of green is directly traceable to the presence of a persistent association formed during the period of exposure (*O.* C.). For *O.* P. the durations in the color series are subject to great variation, being more at the mercy of chance associations than in the case of the gray series. The ascendancy of yellow must be referred for explanation to the following heading (e), since P. reports that it is recalled as a brightness rather than as a color.— It is to be noted here that the colored discs are less reproducible and recordable as *discs* than are gray ones. They often fail to maintain a definite outline, returning as waves of color rather than as distinct images.

e. Brightness. (Ser. V.) An increase both in average duration and in frequency appears toward the bright end of Ser. V (especially noticeable in the record of *O.* P.) The one irregularity in the series is that of black, which is explained by the fact that with this disc a distinct whitish rim, due to an imperfection in the material, seems to have formed the main feature of the image. In general, however, both observers report that the reproduction of the duller shades of the series was less satisfactory, and, in their estimation, less accurate. This is possibly less a matter of direct correlation between reproductivity and the brightness scale, than of the relation of the different shades to the background in the exposure or reproduction period. The black screen and the darkened field of vision would throw the lighter discs into relief, while the darker discs might be less distinguishable from the background in reproduction. According to the laws of involuntary attention, the contrasting image would, of course, lay the stronger hold on the attention during the reproduction period. On the other hand, the longer duration of the image for white may be related to the peculiar value of bright light as an incentive to the attention.

A word may here be inserted on the disturbing influence exerted by affective tone, contrast, and the like. Occasionally, the observer reports somewhat ambiguously, of a figure, "more pleasing," or "more interesting, hence easier to recall." As a matter of fact, we find that, both in total number of recurrences and in average duration for single images, C.'s favorite colors, blue and green, hold their own over all other figures except the notched square. This so-called 'pleasantness' is, however, largely reducible to richness of association.

The effect of contrast, novelty, and familiarity on the course of the images is indicated both in the record and in the introspective account of P. As a result, apparently, of repetition, the images of the standard size medium gray disk (which had a place in Ser. I, III, and V) declined in vividness, and toward the end of the investigation altogether failed on three occasions to appear at all. On the other hand, the image of the largest disc (Ser. III), which rarely failed to elicit surprise, and the qualification "large" or "larger," maintained throughout one of the highest duration values.

On the whole, then, so long as duration is made the criterion, the results do not warrant the hypothesis of any direct correlation between reproducibility and size, outline, color, etc., *as such*. It seems probable that these five factors are varyingly effective only in conditioning the attention bestowed on the figure (presumably during the period of reproduction), hence indirectly affecting the duration of the image. On the other hand, distinctness and completeness of image are, as the introspections indicate, less subject to disturbance from outlying factors, hence more purely indicative of the grade of reproducibility in any case.

The problem will, therefore, next be approached from the standpoint of these more significant factors, and their relation to duration examined.

III. *Completeness and Distinctness*. 1. Completeness. All the records show that completeness is not a function of duration. The briefest image may be as complete as that of lengthiest duration. Further, scrappiness of figure would seem to be correlated both with the direction of attention during exposure or reproduction (see I, 3), and with the narrowed range of attention in reproduction. The portions of the visual field lying in indirect vision in the world of perception, or occupying merely the margin of consciousness, seem, in the world of images, to be totally obliterated, or at most present only in the phantom form in which, in popular phrase, the old moon is said to be present in the new moon's arms. This is illustrated by the report of both observers that when only a crescent out of a whole disc, or the corners of a square, were present in clear consciousness, they could not assert that the remainder of the figure was absent, seeming often to 'feel' rather than to see it.

2. Distinctness. After each record period, a note was made of the relative degree of clearness of the boundaries of the memory image, usually in the terms 'clear' or 'distinct.' Comparison of these observations with the corresponding time records shows that distinctness and duration do not run closely parallel. The individual times in the records reported "unusually distinct" are rarely longer than those for the same figure when reported indistinct. *E. g.*, in a record for the triangle reported "only fairly distinct," the images persist from 2.4 to 4.4 sec., whereas the average duration for that figure is only 1.4 sec. Again, in a record for the square reported as "clear and distinct," the times run from 0.4 sec. to 1.4 sec., while the general average for that figure is 1.8 sec.

At the same time it must be admitted that there is a pretty close relation between the *average* length of duration and the average degree of distinctness for any figure. Thus in P.'s record the figures yielding the longest average duration of image show, with one exception, the largest total of judgments 'distinct.' *E. g.*, the images of the triangle and of the square in Ser. I, the times for which stand at the head of the series, are alone almost invariably reported distinct. In Ser. II, the disc, which has the lowest average duration, has also the majority of judgments 'indistinct.' In Ser. III, size 1 receives the largest number of verdicts 'distinct,' and has also the highest average duration (with the exception of size 4, which is thrown out, on the ground of disturbing associations). In C.'s record this correspondence is less marked, average frequency rather than duration of image varying with distinctness.

It may also be noted in passing that, for distinctness as for duration, angular figures are in the long run more favorable than curved, or, more generally stated, complex are more favorable than simple.

On the whole, then, we may say that, while the distinctness of the individual image is not dependent on its duration, the fact that the image which is typically most distinct tends on the average to maintain itself longest in consciousness suggests two things. Either distinctness is one of the determining factors of duration, or—and this statement seems the more intelligible—both distinctness and duration are dependent on some more fundamental factor. That such a fundamental factor may be found in the capacity of the figure for securing automatic adjustments of the attention during reproduction (or exposure) is suggested by two sets of facts. First, by the correlation existing between distinctness and duration, and complexity; secondly, by the correlation between variations in distinctness and duration for the same figure, and the presence or absence of such incentives to the attention as contrast, novelty, pleasantness, familiarity. When it is recalled that the image during the reproduction period is left entirely to the play of the involuntary attention, it becomes readily conceivable that distinctness is dependent primarily on the presence of one or more features in the figure (*e. g.*, an angle) capable of offering a point of support to the attention, secondarily on reinforcement through some central factor, associative or otherwise; that *duration* of single images is also determined in part by the number of component parts possessed by the figure, and capable of arousing interest in turn; and lastly, that *total duration* or frequency is dependent on distinctness as above determined and is hence a general exponent of the vitality of the image in reproduction. For instance, the square, which presents four definite points or features to the attention in contrast with the monotony of the disc, shows a more persistent tendency to outcrop in consciousness, a greater vitality in maintaining itself, and even when incomplete with reference to the original a greater definiteness of contour than any other member of the series (see Ser. I, *O.* P.).

SUMMARY FOR GROUP I.

The data here collected are negative so far as any immediate correlation between duration or excellence of reproduction and any of the five peripheral factors here considered is concerned. All indications point rather to the significance of central conditions, either in the recording or in the observation period, as the critical factors in determining the character, duration, and frequency of the image. To these central conditions the peripheral factors stand in manifold and varying relations, thus indirectly affecting reproduction.

GROUP II. INVOLUNTARY METHOD WITH EYE-MOVEMENT.

This variation of the procedure was undertaken in order to ascertain whether the introduction of eye-movements during the period of exposure would materially alter the typical results of Group I. More explicitly, the object was to determine whether a yielding to the natural tendency toward eye-movement during exposure, and the consequent increased tendency toward motor innervations during reproduction, would exert any influence on the distinctness, duration, or completeness of the image; and, if so, whether this influence varies with the amount of movement. Group I was designed to test the relation of purely sensory factors to reproduction; Group II, that of the other peripheral factor, the motor element.

In this connection, two passages from articles to which reference has already been made may be quoted. Meakin, in more or less explicit support of the Münsterberg theory of the correlation of vividness with the discharge of motor cells, observes that "the mental tracing of a particular boundary seems to condition the sense of the corresponding contents;" and again, "the effect of the activity of the motor elements of the internal impression is to increase distinctness and prolong duration; the sensory processes standing in intimate dependence on the motor." Slaughter's statement, in his study of mental images, is, while somewhat ambiguous, substantially similar. The factors which keep visual images in clear consciousness are, he says, "their own internal organization closely combined with motor elements." Whether these statements are valid or accurate except under peculiar conditions of experimentation is a question on which the following experiment may throw light. It may, however, be suggested in advance that the question whether motor innervations *per se* condition the presence or distinctness of images would seem to be beyond the reach of ordinary introspective methods. For in so far as any correlation between vividness and innervation is open to observation, *i. e.*, when the kinæsthetic elements corresponding to the latter rise above the limen of sensation, it is obviously open to question whether these sensations do not themselves constitute the reinforcing factor.

It may here be noted that the results already obtained show a correlation between vividness and innervation, but only through the medium of the sensations attendant on innervation. These sensations are, moreover, only those accompanying fixation, hence common to all the figures, and apparently varying only in intensity. That is, there is no exact correlation between the character of the innervation and the outline of the figure, such as a theory like that of Meakin's presupposes. It was thought possible, however, that the previous conditions of experimentation, *i. e.*, rigid fixation, might have been unfavorable to the setting up of the necessary associations, and hence to the highest possible grade of distinctness. The procedure with free eye-movement might, on the other hand, be expected to yield a definite correlation between extent or complexity of outline, and duration or distinctness, if such correlation exists.

A. With the Figures of Group I.

METHOD.

A set of more or less tentative observations was taken with each observer, the directions being freely to explore the outline of the figures presented. The figures used and the general order of experimentation were as in Group I. A full introspective account of each minute's record was kept. For purposes of comparison a single set of observations with fixation, with similar pauses for introspection, and at about the same stage of familiarity with the figures, was given.

RESULTS.

TABLE II.

	Obs. C.								Obs. P.							
	Eye Movement.				Fixation.				Eye Movem't.				Fixation.			
	(1) Av. dur.	(2) No.	(3) Av. dur. for ser.	(4) Av. dur. for set.	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
I. Disc	2.7	4	2.2	2.1	2.1	4	2.3	2.2	1	5	2.2	1.5	1	6	1.3	1.7
Ellipse	1.8	5			2.6	3			2.7	5			1	4		
Square	2.5	4			2.3	5			1.7	8			.7	9		
Triangle	2.1	4			2.3	5			3.7	4			2.6	4		
II. Disc	2.1	5	2		3.3	3	2.6		1.1	8	2.5		.9	7	1.2	
Ellipse	2.1	5			2.6	4			6.9	4			2.3	5		
Square	2.2	5			2.1	4			1.8	9			1	7		
Triangle	1.7	4			2.4	4			.3	4			.8	9		
III. Size 1	2	5	2		2.1	5	2.1		.9	9	1.1		.5	10	1.2	
2	2.3	4			2.2	4			1.2	6			.4	3		
3	2	3			2.1	4			1.1	7			.4	8		
4	2	2			2.2	3			1.2	8			3.7	6		
IV. Red	2.5	4	2.4		2.5	4	2.2		.9	7	.8		1.3	1.2	2.9	
Blue	2.9	5			1.6	6			.5	5			2.5	6		
Green	2.1	4			2.4	4			1.1	6			4.3	5		
Yellow	2.2	4			1.3	5			.6	8			3.7	3		
V. White	2.5	6	2.2		1.9	5	2.1		.3	6	1.2		.8	8	2.2	
Light gray	2.3	5			2.3	5			3.9	4			3.1	5		
Med. gray	2	5			2.5	4			.2	3			2.6	6		
Black	2.1	5			1.9	4			.6	4			2.4	8		

Columns 1, 2, 3, and 4 give the average duration of single memory images, number of images per minute, average duration of image for series, average duration for five series, respectively.

I. The five factors in relation to distinctness and duration. There is even less evidence of any proportionality between duration or distinctness, and size, brightness, amount of outline, etc., than in Group I. None of the five factors seems to have any intrinsic value in reproduction.

II. Movement *versus* fixation, in relation to duration and distinctness.

1. Duration. Comparing the results from the two sets, we find the average duration slightly longer for the procedure with fixation,—1.7 sec. as against 1.5 sec. for P., 2.2 sec. as against 2.1 sec. for C. The average duration for Ser. I and II for P., and for Ser. IV and V for C., is, however, shorter for fixation than for eye-movement, showing no uniformity for the two observers in the alterations effected by eye-movement.

2. Distinctness. The introspective evidence for the two observers is more in accord. First, for distinctness and completeness of image the advantage is with the method of fixation. Secondly, the cases of lengthened duration with eye-movement are correlated, not with greater stability of image, but with the tendency to explore the margin, and with the successive appearance and disappearance of the component parts of the figure. With the new procedure, moreover, the localization of the memory image is less definite, more often to one side of the point of fixation; while the image itself is less stationary, and often seems to float across the field.

These results seemed, on the whole, sufficiently promising to warrant a repetition of the procedure under more favorable conditions; namely, with a set of figures free from the habitual fixation associations of the former method.

B. With New Figures.

MATERIALS.

The new series consisted of four figures, of the standard gray tones, of approximately equal surface, and as little suggestive of definite associations as possible. The series was as follows:

1) Pentagon.
2) Octagon.
3) Rectangle.
4) Quadrilateral. (An elongated diamond.)

This series was given twice each observation period, once with fixation and once with freedom of eye-movement during the four seconds' exposure. Steady fixation was facilitated by the use of a head rest. There was also a slight alteration in the manner of exposure, the observer closing his eyes for a second during the removal of the screen, in order to lessen the tendency to eye-movement on exposure. Further, the record of the course of the images was begun five instead of twenty seconds after the exposure of the figure, it being thought desirable to make use of the images which always occurred during this pause, and trial having indicated that the observer could practically abstract from the appearance of the sensory after image. Four repetitions of the whole procedure were given with observer C. only.

RESULTS.

TABLE III. OBS. C.

	Eye Movement.						Fixation.					
	(1) Tot. dur.	(2) Av. dur.	(3) M. V.	(4) Av. No.	(5) Tot. dur. for ser.	(6) Av. dur. for ser.	(1) Tot. dur.	(2) Av. dur.	(3) M. V.	(4) Av. No.	(5) Tot. dur. for ser.	(6) Av. dur. for ser.
Rectangle,	40.7	2.2	.5	4.5	166.2	2.1	38	2.1	.4	4.5	150.1	2
Quadrilateral,	49.4	2.1	.3	5.6			36.8	2	.3	4.5		
Pentagon,	38.2	2.1	.3	4.5			35.7	1.7	.3	5.2		
Octagon,	37.9	2.1	.3	4.5			39.6	2.3	.4	4.2		

In the above Table columns 1 and 5 give the total duration of image for each figure and for the series as a whole (for four observations); 2, the average duration of image; 3, the mean variation; 4, the average number of images per minute; 6, the average duration for series.

I. *Introspections.* (For eye-movement.) 1. Incentives to reproduction. Occasionally the image seems to be overlooked by some chance eye-movement, as in winking. Moreover, eye-movements, actual or imaged, play a considerable part in the maintenance and completion of the image.

2. Localization. The memory image is more patchy, illusive, and oscillatory, than in the procedure with fixation. The space relations of the image are less definite (as noted in A.), and the image is more often of the 'subjective' type, with the sensations of fixation lacking. The best, most definitely localized images were obtained only when C. had become sufficiently practised with the material to sweep the figure with his eyes, and have left a fraction of the four seconds of exposure for fixation. This device seemed to come to him naturally as a means of escape from the feeling of dissatisfaction attending a mere cursory exploration of the margin.

3. Distinctness and completeness. In general, the images are described as less clear and definite and less complete than in the procedure with fixation, the pentagon and octagon being especially defective. In the most distinct, portions only are clear. Thus the angles only of the rectangle are clearly defined, the intermediate line and surfaces being hazy or absent, while in the quadrilateral the upper acute angle only may be definite.

II. *Duration and Frequency.* 1. Duration of image for fixation and eye-movement. Comparison of the general averages shows a slight advantage in favor of the procedure with eye-movement, 2.1 sec. as against 2 sec. This (as noted in A) is correlated with the tendency of the image, in the procedure with eye-movement, to rush across the field in indistinct patches, or to build itself up, bit by bit, by real or imaged eye-movement. The averages, also, for individual figures show a longer duration for the eye-movement procedure, except in the case of the octagon. The latter shows a duration of 2.3 sec. for fixation as against 2.1 sec. for eye-movement.

3. Duration in relation to distinctness and completeness. The higher average duration for the procedure with eye-movement is associated with an actual decrease in the distinctness and stability of the image. There is, moreover, no precise correlation between distinctness and completeness, and duration, in the case of any single image. Nevertheless, the rectangle (in the procedure with eye-movement) shows a greater total of judgments 'distinct' and 'complete,' and at the same time a slightly higher *average* duration than any other member of the series. Hence, as in Group I, we may conclude that duration and distinctness, while practically independent of each other, are both furthered, though not absolutely determined, by the same general conditions. The exact nature of these conditions may be brought out more fully in connection with Group III.

SUMMARY FOR GROUP II.

1. The appearance of the image in consciousness is not necessarily dependent on the conscious 'mental tracery' of its boundary. Indeed, under conditions of observation or experimentation which tend to produce such a dependence, the character of the image as a whole is impaired, and the simultaneous appearance of its parts hindered.

2. Fixation during exposure affords the more favorable condition for reproduction,[1] either for the reason that it secures a more impartial distribution of the attention over the figure, hence a clearer impression of the whole, or for the reason that, through the association thus set up between the retinal image and the sensations involved in fixation, these sensations, when repeated or reproduced with the image, constitute a more potent reinforcement of the image than could the fleeting sensations producible by irregular or transitory ocular movements. In short, it is not general ocular movement, as Meakin implies, but certain special motor accompaniments of the state of visual attention, which contribute the effective conditions of visual reproduction.

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[1] At least in the case of figures of the size here employed.

GROUP III. VOLUNTARY AND INVOLUNTARY METHOD WITH FIXATION.

The object of this set of observations was, first, to repeat in more specific fashion the test of the relative effectiveness for reproduction of figures of curved and angular outlines, of relative simplicity or complexity, and of smaller and larger size; secondly, to determine whether the constant differences in duration and distinctness of memory images of different figures are attributable to differences in the incentives offered by the figure to passive attention (during the period of reproduction), or to conditions relatively favorable or unfavorable to reproduction, established during the period of exposure and intrinsic to the image of any particular figure. That is, it was desired to ascertain whether the observed differences between memory images of different figures were intrinsic and necessary, or merely peculiarities resulting from the involuntary procedure thus far adopted in the reproductive period.

MATERIALS.

Three new series were prepared, each consisting of four similar figures, of the standard gray tone, and of graded sizes, each figure being approximately double the size of that preceding. Series I was made up of discs, Series II of triangles, Series III of discs like those of Series I, in which were inscribed (in Indian ink) triangles similar to those of Series II. It was especially desired to eliminate, so far as possible, any disturbances from familiarity and contrast effects, or from associations. Hence the triangle used was an isosceles with an acute vertex, in place of the equilateral used in Group I; and the figures in each series were given in regular, not haphazard sequence, size 1, 2, 3, 4 in turn, in order to lessen contrast.

METHOD.

The procedure was in general similar to that in Group II B., except that the procedure for eye-movement was omitted, and voluntary alternated with involuntary recall. Records of voluntary recall of images for one minute were taken for the whole set of figures, alternating with similar records for involuntary reproduction. The whole set of three series was given in a period, twice in all for each procedure, voluntary and involuntary. One observer only, P. of Group I, was employed.

A few supplementary observations were taken with voluntary recall of a single image, after each exposure. Half were given with fixation, half with eye-movement during the four seconds of exposure. In these the results tallied with those obtained by the ordinary method of recording so far as distinctness, completeness, and ease of recall are concerned. Moreover, differences parallel to those of Group II were observable in the results for fixation and eye-movement.

TABLE IV. OBS. P.

		Involuntary.						Voluntary.					
		(1) Tot. dur.	(2) Av. dur.	(3) M. V.	(4) Av. No.	(5) Tot. dur. for ser.	(6) Av. dur. for ser.	(1) Tot. dur.	(2) Av. dur.	(3) M. V.	(4) Av. No.	(5) Tot. dur. for ser.	(6) Av. dur .for ser.
I. Circles	1.	28	1.4	.5	9	95.7	1.8	83.2	4.6	2.1	9	203.9	2.7
	2.	11.8	1.1	.5	5			41.3	2.1	1.2	9.5		
	3.	28.8	3.2	2.2	4.5			41.5	2.4	1.6	8.5		
	4.	27.1	1.7	.8	8			37.9	1.8	1	10		
II. Triangles	1.	16.8	2.1	1.9	5.2	73.1	2.3	80.4	7.3	2.4	5.2	240.5	4.1
	2.	8.3	1.1	.7	7			65.9	4.7	1.3	7		
	3.	17.8	4.9	4	2			33.8	1.8	1	9		
	4.	15.8	1.2	.5	5.2			60.4	2.7	1.4	11		
III. Inscribed triangles	1.	32.2	2	1.2	8	155.1	2.3	72.9	3.8	1.4	9	240.3	3.1
	2.	39.6	2.6	1.4	7.5			56.4	2.2	.7	12.5		
	3.	45.6	2.6+	1.3	8.5			58.9	3.2	1.2	9		
	4.	37.9	2.2	.9	8.5			51.8	3.2	1.3	8		
		Gen. av. 1.1						Gen. av. 3.3					

In the above Table columns 1 and 5 give the total durations of image for each figure and series, respectively; 2 and 3, the average duration of image and mean variation; 4, the average number of images; 6, the average duration of image for series.

RESULTS.

I. *Introspections.* 1. The incentives or aids to reproduction employed in voluntary recall are: verbal idea of size; recall of some striking feature (as the angle of the triangle, or the black line in Ser. III), with slow construction of the image about this; movement of the eyes toward the point where the object was seen; and, above all, steady fixation.

2. Discs were more difficult to recall than triangles, or discs with triangles inscribed. In the latter figures, the triangles are more distinct than the surrounding segments.

3. The percentage of judgments "distinct" and "complete" for each series is as follows:

	Involuntary.		Voluntary.	
	Distinct.	Complete.	Distinct.	Complete.
Series I.	60%	12%	75%	37%

	Involuntary.		Voluntary.	
	Distinct.	Complete.	Distinct.	Complete.
II.	75%	37%	64%	60%
III.	37%	00%	37%	12%

II. *Relative Effectiveness for Reproduction.* The three points to be tested will be examined here, differences in involuntary and voluntary recall being abstracted from.

1. Curves and angles. The triangle has in general the advantage over the disc, in duration, completeness, and distinctness of memory image, as the Table shows. It is, also, as noted above, more readily recalled, and in Ser. III is usually the most positive part of the figure. That this is less a matter of curves and angles in themselves than of complexity as against simplicity, or of varying incentives to fixation, is indicated here as in the previous groups of experiments.

2. Size. The duration, distinctness and completeness of the memory image are not proportionate to the size of the figure reproduced. For involuntary reproduction the next to the largest size invariably overtops the series, both for individual and for total durations. It is noticeable, however, that the corresponding images are reported incomplete, and only moderately distinct. For voluntary recall, the image of the smallest figure maintains itself longest in consciousness. In both procedures (and this seems to be the significant item), the memory image of the smallest figure excels all others both in distinctness and in completeness. Evidently there is, then, an optimal size in visual reproduction, related either to the retinal area of clearest vision, or to the limited field of vision in reproduction.

3. Complexity. The complicated figures of Ser. III have a very slight advantage over the simpler figures (Series I and II) and that mainly in the number of images to the minute. This advantage is partly to be attributed to the fact that in successive images different parts of the figure were accustomed to appear, thus placing the reappearances on a standing different from that of ordinary repetitions. For average duration these figures stand on a level with the triangles in involuntary recall, below them in voluntary. What is more significant, both in distinctness and completeness they rank decidedly below the other figures. But while in this case complexity seems to exercise an unfavorable influence on reproduction, on taking the triangle as an example of relative complexity in comparison with the disc, we find our conclusions reversed. Hence, we conclude that the complexity of the figures of Ser. III exceeds either the limits within which a figure is easily grasped during four seconds' exposure, or the limits of the range of attention for reproduced sensation.

In summary, then, we may say that the figures of small size and of moderate complexity offer the conditions most favorable to reproduction. Whether these conditions are effective during the period of observation or reproduction, or both, must now be considered.

III. *Differences in Reproducibility in Voluntary and Involuntary Reproduction.* With voluntary recall there is a general redistribution of values, with regard to duration, distinctness, and completeness, for the three series. Since, however, this redistribution fails to reduce the memory images of the three types to the same level, and, moreover, reproduces most of the distinctions noted for involuntary recall, the conjecture advanced in Group I, that the superior reproducibility of a figure is due to some intrinsic or accidental qualification for securing a better adjustment of the passive attention during the period of reproduction, seems insufficient to explain the facts. That is, maximal attention during observation is not as uniformly effective for reproduction in the case of figures as in the case of simple sensations, but is complicated by the relation of the figure to certain central conditions. This inequality in the relations of different figures to the central conditions is one that asserts itself under conditions of voluntary as well as involuntary attention, *i. e.*, during either the period of observation or of reproduction. This inequality must now be more closely examined.

It is first to be noted that in Group III, as also in I and II, constant differences in reproducibility are correlated principally with "complexity" of figure. That the image of the more complicated figure should, either in voluntary or in involuntary recall, remain relatively longer in the focus of consciousness than that of the simple figure might be expected if we were to reason by analogy with conditions existing in perception. Gordon[1] has shown that the duration of a single pulse of attention is longer for a complex than for a simple visual object. It is possible that this rule obtains also in the sphere of reproduced sensations, and that the longer durations of Series III (especially in the voluntary procedures where the interruptions in the appearance of the memory image have often the character of fluctuations) are further expressions of this rule. Yet the fact that the most complex figure (that of Ser. III) does not give the longest memory image warns us that this function of complexity is not to be accepted without limitations. The existence of an optimal complexity for reproduction indicates that it is not complexity itself, but the extent to which it affords conditions favorable to fixation and attention, which is the determining factor. Moreover, the above quoted facts on the pulse of attention would hardly lead us to anticipate that the image of a complex figure would be not only longer, but also, within certain limits, more complete and distinct than those of other figures. Hence, it seems necessary that the facts be analyzed further in order to arrive at the precise nature of the advantage conferred (in certain cases) by complexity.

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[1] K. Gordon: Meaning in Memory and Attention, *Psych. Rev.*, X, 267.

Let us look first at the period of exposure for some influence exerted in association with complexity either to enhance the neural excitation underlying reproduction or to furnish associated factors which may serve to reinforce the reproduction. The sensations arising from the innervation of the muscles used to inhibit those eye-movements to which the corners of the figure form incentives seem to offer precisely the factor required. The introspections show a tendency to movement, which is felt and resisted, both in the period of exposure and of reproduction, and which bears a significant relation to the presence and distinctness of the image in the latter period. Since with the simple disc there is apparently no corresponding tendency or resistance, it seems probable that the difference in distinctness and duration of the triangle as compared with the disc is due to the presence in the visual complex of associated fixation sensations of greater intensity, set up again on the appearance of the retinal image in memory, and acting as a reinforcement ot this image. In brief, the figure which gives rise to the clearest and strongest fixation sensations has, other things being equal, the best chance in reproduction.[1]

The doubt may of course be raised as to whether these kinæsthetic sensations are actually reinforcing factors, or merely coefficients of the efficiency of attention. The weight of evidence, however, throughout all these groups of experiments, seems to lie on the side of the former hypothesis.

SUMMARY FOR GROUP III.

1. There is an optimal size and complexity for visual reproducibility, dependent on the range of atttention.
2. Conditions obtaining in the period of exposure are critical for reproduction, since certain differences in reproducibility are constant both in voluntary and in involuntary recall.
3. Along with various central factors (familiarity, contrast, association and the like) conditioning the appearance and dis-

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[1] *Cf.* Wilhelm Peters: Aufmerksamkeit und Zeitverschiebung in der Auffassung disparater Sinnesreize, *Zeitschr. f. Psych. u. Physiol. der Sinnesorgane*, Bd. XXXIX, 1905, 427, for the value of fixation elements in visual attention.

tinctness of the image, the kinæsthetic elements of fixation play an important rôle.

The results of the investigation may be summed up as follows:

Neither the attributes of the stimulus, qualitative or spatial, nor the general ocular movements to which these attributes may give rise, constitute the important differential factor in visual reproduction. On the contrary, reappearance and persistence, distinctness and general accuracy of reproduction are conditioned primarily upon the relation of the stimulus or image to central conditions, and upon certain special motor phenomena accompanying fixation.

FURTHER STUDY OF THE ENGLISH SPARROW AND OTHER BIRDS.

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INTRODUCTION.

In an earlier paper on "The Psychology of the English Sparrow" the writer committed himself to the further study of the same and other species. Like most programmes, perhaps always more or less unwisely made, this has seemed tardy of fulfillment. For experimenting with such frail and nervous animals one must have an unlimited amount of time and the possibility of making the surroundings very free from disturbing noises, etc. It is scarcely necessary to say that neither of these conditions could be fully satisfied. The birds have been kept and the experiments tried in a large animal room with alligators, white rats, and Jack the monkey, in other cages near by. These were often a disturbing factor while an experiment was in progress. Then, too, the necessity of carrying along simultaneously the same test with a number of birds led to a change in the method from that used earlier; at least, during the first experiments. To do this each bird was kept in a small cage outside the large one (the latter being twelve feet long and wide and six feet high). When tests were made these cages with the birds were carried inside the large one. Later they were allowed to go directly from the small cages into the large one. This latter method it seems to me is much better as it eliminates the fright which always results from carrying the small cage and the birds become more accustomed to the large cage. They are not suddenly confronted with so much that is new and consequently there is less distraction. Notwithstanding the fact that the same precautions as to food, air and sunshine, have been taken as in the earlier study I have found it difficult to keep the birds in good condition for any great length of time. Since from the first I was on the sharp lookout for abnormal conditions of any kind, allowance will be made for these in the interpretation of results.

DESCRIPTION OF BIRDS EXPERIMENTED WITH AND WORK DONE BY OTHERS.

The birds experimented with were a Vesper Sparrow, a female Cowbird, a male Dove-cot pigeon, a female Passenger pigeon, and some half dozen English Sparrows. The results of a few tests on a Red-headed Woodpecker will also be described. Some facts by way of description of the species, their habits and habitat, and of interest chiefly from a psychological point of view, will now be given.

The Vesper Sparrow, Grass Finch or Bay-winged Bunting is somewhat smaller than the English Sparrow and not so strongly or roughly built. According to Chapman (1, p. 141, and facing p. 122 for picture) its habitat is the great broad fields where it finds its food. It is protectively colored, the upper parts being grayish black and brown; breast and sides streaked with black and brown; belly light; lesser wing coverts chestnut. It has a very sweet song, some parts of which as well as the sharp, clear calls were to be heard in the laboratory. Its habit of feeding and flying near the ground is so strong that it required some patience and several trials to get it to leave the large cage by an opening in the upper corner. Its scratching habit was so persistent that to the last, after months of captivity, no food could be kept in its food-box.

From the standpoint of its habits and instincts the Cowbird is one of the most interesting and hated of birds. It is somewhat larger than the English Sparrow, being rather an obscure looking bird of dusky grayish-brown color. This is particularly true of the female. See Chapman (1, facing page 114) for colored picture. Like the European cuckoo it builds no nest of its own but lays its eggs in other birds' nests. According to Coues (2, p. 402), "it is migratory, abundant, gregarious, polygamous, polyandrous, and parasitic. . . . It appears to constitute, furthermore, a remarkable exception to the rule of conjugal affection and fidelity among birds. A wonderful provision for the perpetuation of the species is seen in its selection of smaller birds as the foster-parents of its offspring; for the larger egg receives the greater share of the warmth during incubation and the lustier young Cowbird asserts its precedence in the nest; while the foster birds, however reluctant to incubate the strange egg (their devices to escape the duty being sometimes astonishing), become assiduous in their care of the foundling even to the neglect of their own. The number of species thus imposed upon is now known to be about one hundred. In the western prairies great flocks of this bird may be seen following herds of cattle. They very often feed upon the parasitic insects found on the backs of these cattle. When kept in captivity with other birds the Cowbird, according to Prof. C. F. Hodge and Mr. Raymond, performs the same good office for them. My Cowbird I have never seen do this, but she had the habit of approaching quite near to the English Sparrows and looking them over very carefully. Does this act relate itself to the food-getting instinct or does it have its setting in the friendly stealthy attitude and approach which this bird has in order to lay its eggs in other birds' nests? Another very interesting performance was the turning of her head on one side and moving it round as though to take in the whole expanse of sky. She did this most often immediately after alighting. This has not been seen in other birds which I have observed. It would seem to find its explanation in the necessary watchfulness she must exercise in order to deposit her egg in the nests of other birds without being seen.

The Pigeon is so familiar that just a word of description will suffice. Schmeil (12, p. 195) writes as follows: "The races of our domestic pigeons are the descendants of the rock-pigeon which inhabits the cliffs of the coasts of the Atlantic and Mediterranean and other similar places. It has been domesticated from times immemorial. By means of constant selection numerous races or varieties of pigeons have been produced which frequently exhibit in shape, color, formation of beak, feet, etc., more considerable differences than distinct species of birds. The pigeon has a keen sense of sight. It is a defenceless bird and is consequently *shy* and *timid.*" Yet in this respect Professor Mills (6, p. 257) makes a difference between the different varieties of pigeons. He says "though differences between the mature forms of varieties of pigeons, so pronounced as regards physical form, less so psychically, but still real and always present, are obvious to even a superficial observer, it is interesting to note that even at an early date such differences do appear. To illustrate: the Dragoon is a bird of very bold appearance, and as compared with many varieties, is somewhat wild. It has been spoken of as the 'game bird' of the pigeon family. Such characteristics are manifest in the young before they are twenty days old. They peck sooner and more vigorously in the nest. They are shyer of approach, etc. This cannot be explained by a more rapid development, for several other varieties mature sooner than they do."

The English Sparrow and its habits are, like the pigeon, so familiar that it needs but little description. For this the reader is referred to my earlier paper (9, pp. 313-316).

A few words will suffice to set forth all that has hitherto been attempted in the investigation of the psychical differences between birds of the same or different species. Professor Whitman (15, pp. 331-338) has made experiments which point to a difference in intelligence between the three varieties of Pigeons —the Homing, Ring-neck, and Dove-cot. The first experiment consisted in placing the egg just outside the nest. Would they have intelligence enough to modify their instinctive way of reacting and thus pull or try to pull the egg back into the nest? The Passenger Pigeon recognizes, not by sight but by feeling, that something is wrong. Her instinct being keenly attuned she promptly leaves the nest. The Ring-neck may try to reclaim one egg. Having done so she is satisfied. The Dovecot tries to reclaim both eggs and failing leaves her nest with more hesitation than the others.

Here, then, according to this author, is a difference between the power of intelligent action to be found in these birds. The reason for this difference, according to Prof. Whitman, lies in the fact that domestication has let down the bars to alternative choice. And this means the beginning of intelligent action. The wild or semi-wild pigeon has always nested in a place where it has never recovered its eggs after they were once out of the nest. With the domesticated pigeon conditions have been different. It could reclaim its egg.

Prof. Whitman also notes that the Ring-neck has great difficulty in locating her nest-box even though it has been moved but little out of its accustomed place and everything else remains the same. She depends entirely on its relations to the environment.

The above constitutes the sole experimental contribution, so far as the writer's knowledge goes, to the mental differences between birds. Whether or not we agree that Prof. Whitman has placed the correct interpretation upon the fact which he has found is a question aside. However, we are able to see that he finds by experiment and observation that there are these differences in intelligence and variability of instinct between his three varieties of pigeons. Domestication and semi-domestication are responsible, according to him, for these differences. Now, no one will question the statement that the English Sparrow has been for thousands of years in just as favorable, indeed, if not more favorable, conditions with relation to domestication. As stated in a former paper, it has retained its native wildness, and yet lives with man, compelling the latter, in spite of his efforts to the contrary, to feed it and give it shelter. That these are good conditions necessary for the development of variability or alternative choice there can be little doubt. Apropos of an investigation into the mental differences between birds not only into the cognitive but the emotional and volitional as well, the following from Chapman (1, p. 137) is not without interest,—"Even after leaving the nest the parasite (the cowbird) continues its call for food, and when seeing a Maryland Yellow Throat, or some other small bird feeding a clumsy fledgling twice its size, one wonders it does not detect the deception. The better we know birds the more strongly are we impressed with their individuality. To one who has no friends in feathers it seems pure fancy to endow some insignificant "chippy" with human attributes; but in reality there are as clearly defined characters among birds as among men. To be convinced of the truth of this statement we have only to compare the Cowbird, a thoroughly contemptible creature, lacking in every moral and maternal instinct, with the bird who constructs a well-made nest, faithfully broods her eggs, and cares for her young with a devotion of which mother love is alone capable." What additional evidence can controlled observation and experiment offer as to mental differences among birds?

TESTS WITH A SIMPLE MAZE.

After making several trials with Dr. Small's complex maze, a reduced form of which was used in my earlier work, I concluded that it was too difficult for a beginning test as well as too great a tax upon my time, a single test sometimes requiring as much as four hours' time. Accordingly a simple maze was made. Fig. 1 shows the plan of this maze which was four feet square with alleys five inches wide and high. The bottom and top were of the same wire mesh both being cut away at C, the centre, leaving this open. The partitions and sides were fastened by unravelling the wire mesh of the edges and clinching these into the top and bottom along their entire length. This maze is therefore very free from landmarks but since it is so simple this is not of so much consequence as in the more complex one. It will be seen that there are two longer ways, and one short one, to the centre. The maze was placed in the middle of the floor of the large cage and food at C. The small cages were placed with open door at O, the opening to the maze. After reaching C the bird was free to eat and fly about in the large cage. There were thus two incentives to cause the birds to put forth greater efforts in order to get to C; namely, the desire for food and for the greater freedom afforded

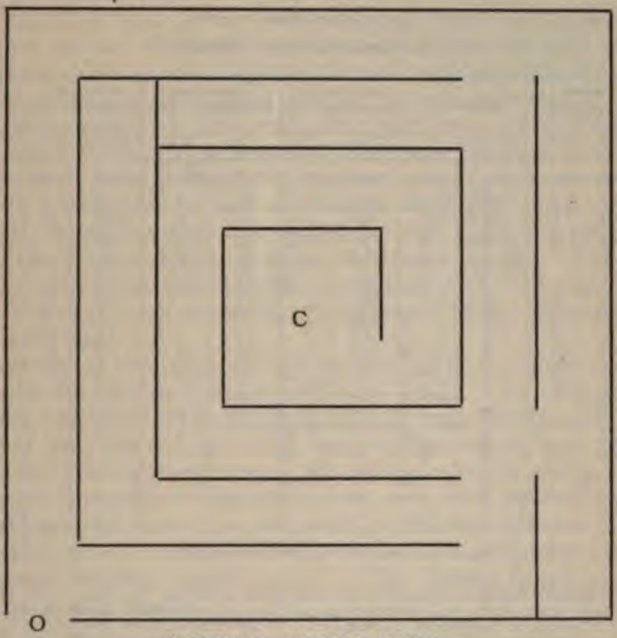

FIGURE I. SIMPLE MAZE.

by the larger cage. The birds were recaptured by hanging the small cage with open door on the outside of the larger one and carefully driving them through a small door in the upper corner of the larger cage. Six different birds were tested with this maze. Each approximately the same number of times daily. The birds were a Vesper Sparrow, a female Cowbird, two male and two female English Sparrows. The table following shows the results for a series of thirty tests, the numbers under "Time" and "Errors" being averages of each two consecutive trials. The time is given in seconds and an error means a retracing, no matter how slight, in the wrong direction or toward the starting point. A record of the path taken by the bird was made by tracing a line through a reduced plot which I held in my hand. F means failure. F–F means failure both trials of the first two tests of the Cowbird. F–880 indicates that there was a failure the third trial and the fourth trial required 880 seconds. Other cases so indicated are to be interpreted in like manner. Ch. indicates the number of reversals from the short to the long way or *vice versa*.

The Cowbird and a male English Sparrow failed to get through the maze during the first trials. Their claws seemed to hook round the wire in the floor of the alleys so that they

TABLE I.

Tests with Simple Maze.[1]

No. of Trial.	Vesper Sparrow.		Cowbird.		English Spar'ow. F.5		English Sparrow. M.6		English Spar'ow. F.7		English Spar'ow. M.8	
	Time.	E'rs.	Time.	E'rs.	Time.	E'rs.	Time.	E'rs.	Time.	E'rs.	Time.	E'rs
1–2	44	3·5	F–F	91	116	7·5	F–504	58–45	645	121	130	11
3–4	112	3·5	F–880	F–27	32	1	59	2	133	13	91	7
5–6	30	1	231	44	36	1·5	56	0	78	6	20	2
7–8	44	4	F–420	6	170	7·5	68	4·5	16	·5	14	1
9–10	31	·5	F–57	7	12	0	36	0	14	1·5		
11–12	26	1	27	1	22	1·5	13	0	11	0		
13–14	43	8	13	1	88	0	13	·5	11	1		
15–16	39	3·5	12	1·5	98	2	7	·5	7	1		
17–18	13	1	6	·5	16	0	124	2	14	0		
19–20	26	2·5	8	2	14	·5	10	0	9	0		
21–22	10	0	6	·5	7	0	15	·5				
23–24	8	·5	5	·5	13	0	9	0				
25–26	11	·5	7	·5	23	1·5	7	0				
27–28	9	1	4	0	9	·5						
29–30	13	1·5	5	·5	9	·5						
Ch.	15		2		3		1		2			

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[1] In this and all following tables the *time* is given in seconds.

had considerable difficulty in getting along at all. It will be seen that the English Sparrow overcame the difficulty more rapidly than the Cowbird. The fact that the latter's claws are more hooked may very well account, in part, for her greater difficulty. Results from other experiments would agree with this. She probably is also less rapid in adapting herself to new situations. Her later progress in learning the maze, as indicated by the table, shows that it was probably nothing more than a late start.

The Vesper Sparrow, during all the trials, and especially during the first one, seemed most at home of any of the birds. To one who knows the native haunts and habits of this bird this has seemed surprising. But from a bird that lives and nests near the ground amid thickets and clumps of bushes this is just what I should expect. Her habit of staying very near the ground was shown very clearly when I tried to drive her from the large to the small cage. She persisted for some time in flying about near the floor. Furthermore, the Vesper Sparrow was throughout less easily frightened than the others. This would be an advantage to her during her first trials in the maze. She, the Cowbird and the English Sparrow, No. 7, became quite tame as the experiments progressed. The Cowbird wanted to come through the maze before I could get out of sight. But the tameness of the English Sparrow was probably due to his abnormal condition as he died a few days later. On the contrary, the female English Sparrow, No. 5 (which I pronounced to be a young one, judging from the yellow bill, the color of her feathers, and her size), became very much frightened, and at the slightest disturbance she would crouch at some place in the maze instead of going through. Her fright was caused by her escape into the large room from the small cage. She had to be caught and this was done only after a prolonged chase. English Sparrows learn much more readily than other birds to take advantage when the small door is opened. Her wariness was shown by the time she remained in the small cage before entering the maze at all. This was considerably greater and more persistent with her than with the others. A later series with this same bird was discontinued on this account.

A glance at the table will show that all of the birds learned the maze during the twenty or thirty trials. All of them did not take the short way through, but if some habitually went the long way this did not lengthen the time much and should not be counted against them. But if there were many reversals from the long to the short way and *vice versa* then this should be noted since it would seem to indicate either a failure to discriminate or, what is more probable, an incapacity to hold in memory the way last travelled. The Vesper Sparrow made thirteen such reversals in thirty trials, the Cowbird three, the female English Sparrow, No. 5, four, the male English Sparrow, No. 6, one, and the male English Sparrow, No. 7, two such reversals. These reversals with the Vesper Sparrow grew less frequent toward the last of the series. Taking the results just mentioned in connection with the reduction of the time required and the number of errors, the Vesper Sparrow would seem to have the poorest record of any. If there is any difference between the Cowbird and the English Sparrow it is slightly in favor of the latter, particularly at the start. This is not so noticeable in the time as in the number of mistakes made. One reason for the Cowbird's short time is that she chose the most direct way and went very rapidly.

It can be seen from the table that the number of errors does not always increase with the time. The early adaptation of all these birds, and, according to these results, especially of the English Sparrow, is one of the chief points of interest. Many of the errors were very slight, and although they were corrected at once, record was always made of them. Habit seemed to hold them to their mistakes just as it does with us, and recognition of a certain part of the maze through sight or muscular sense, or both, seemed to be the cue for retracing their steps.

MEMORY TESTS.

Following an interval of thirty days a series of ten memory tests was made with three birds, the Vesper Sparrow, the Cowbird and the female English Sparrow, No. 5. There were no intervening tests and all the conditions were as nearly as possible the same as for the initial series. The results are given in Table II which is to be interpreted in the same manner as the table preceding.

TABLE II.

Memory Tests with Simple Maze.

No. of Trials.	VESPER SPARROW.		COWBIRD.		ENGLISH SPARROW. Fe. 5	
	Time.	Errors.	Time.	Errors.	Time.	Errors.
1–2	17	1	7	1	17	1
3–4	49	1.5	4	0	10	5
5–6	26	2.5	5	0	15	1
7–8	7	.5	7	.5	19	5
9–10	13	.5	4	0	9	0

The results in this table show that birds have a very good memory. The Cowbird shows the best results, her average time for the memory tests being the same as for the last ten trials of the first series. There are also fewer errors in the memory series. The average time for the other two birds is not quite equal to the last ten trials of the initial series but is better than the second ten of this series. The Cowbird seems to have forgotten little or nothing, the other two forgetting more of what they had learned a month previous but by no means enough to place them in their earlier inexperienced condition. Here again the differences between the individual birds are so slight, especially between the Vesper Sparrow and the English Sparrow, that it is well not to make too much of them.

Immediately following these memory tests the maze was reversed thus compelling the birds to enter from the opposite corner. In this table the individual results for each trial are given. I have not averaged each two successive trials. It can

TABLE III.

Tests with Maze Reversed.

No. of Trials.	VESPER SPARROW.		COWBIRD.		ENGLISH SPARROW. Female. 5.	
	Time.	Errors.	Time.	Errors.	Time.	Errors.
1	554	33	135	27	780	24
2	33	1	15	4	120	5
3	31	2	11	1	41	2
4	35	4	38	5	106	5
5	36	3	10	2	62	2
6					23	1

easily be seen from the accompanying table that the effect of this was to upset entirely the habits previously formed. The beginning times and errors are very high. At a certain turn in the maze they tried very hard to go in the old direction. This was rapidly overcome as the succeeding trials clearly show. The behavior of the birds in this experiment tends to strengthen the opinion formed from earlier experiments with the more complex maze; namely, that, especially after the maze is learned, the birds do not depend on sight alone for their cues as to when to turn and in which direction, but on a sense of direction and distance as well. That this is, at least in part, in terms of muscular sensations is probable.

TEST WITH FOOD-BOX WITH PIGEONS AND COWBIRD.

The box used was not identical with the one used in the earlier study with the English Sparrows alone. It was thought advisable to make it considerably larger, especially for the pigeons. This box is twenty inches long, thirteen inches wide, and twelve inches high. The latch L is on the inside and when let down the door closes by means of a small catch which is fastened to the door. This latch may be easily raised by pushing or pulling any one of the strings A, B, C, or D. A spring at S pulls the door open when the latch is raised. The end to the left and the entire front are of wire mesh, so that the food is easily visible from most points in the large cage. The top and the other side and end are solid. When experimenting the right end was placed against the wall of the large cage. As in the earlier tests, the birds were fed in this box with the door open for at least two days before the experiments proper were begun. During most of the time this box was removed after the bird had eaten. This gave the birds the freedom of the large cage during the interval between tests, a very desirable condition to fulfill if it is at all possible. In taking the time the watch was not started until the birds

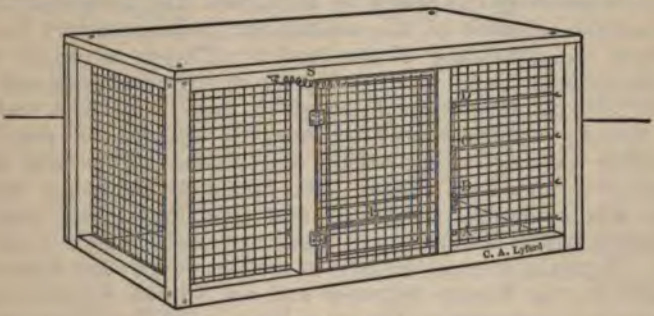

FIG. 2.

reached the floor near the box or were standing on the box itself. As in all other tests previously described, a record was kept of the number and kind of errors made. These, however, do not appear in the table. The tests were made several times daily, varying from two to nine in number. This number was practically the same for the different birds. In the table below, which is to be interpreted in the same manner as Table I, will be found the results obtained with the Cowbird, a male Pigeon and a female Pigeon. The female Pigeon was some five or six years old and of the Passenger variety. The male was younger but apparently had attained his full size and growth. He was of the common Dove-cot variety. I could do nothing more than merely begin a series with the young English Sparrow, No. 5, her fright by this time had caused her to become so unmanageable. The other English Sparrow, No. 8, died before I could try her by herself, and still another one fell a prey to gray rats.

As is probably the case with all tables, the notes describing the actual behavior of the animal are quite necessary for the correct understanding of the results. This is especially true in the present case. The description of the conditions and the results for the Cowbird will be given first. Her first method of opening the door was by hopping up on the side and pushing in the strings with her claws. This was done entirely at random during the first and following trials. She was somewhat frightened and consumed much time in standing before the strings in a half crouched attitude ready to act. To make it easier for her, as well as to see what they each would do, the Vesper Sparrow and an English Sparrow were turned into the cage at the same time. But the Vesper Sparrow was soon removed because she showed little disposition to get into the box, and also because of the vociferous attention which she paid to the others. I was not able to satisfy myself as to whether this was a manifestation of hunger or sexual excitement. It was probably the latter. She later fluttered against the side of the cage and showed a strong desire to escape. She soon died. It was in early spring and her actions were probably those of the mating period. With very few exceptions the English Sparrow was leader. She often made several excursions to the box before the Cowbird reached it. Some few times I have thought the Sparrow was imitating in some slight way the other's method of opening the door, but I could not be sure of this. The Cowbird gradually reduced the number of useless efforts at other places on the box and at the same time learned to use the bill instead of the claws. She also dispensed with the hopping up on the side of the box and remained standing on the floor while opening the door. This is

TABLE IV.

Experiments with Food-box.

No. of Experiments.	COWBIRD.	PIGEON, Male.	PIGEON, Female.
	Time.	Time.	Time.
1–2	140	345	397–F
3–4	570–F	F–670	248
5–6	F–125	F–240	101
7–8	220	810–F	72
9–10	630	F–550	25
11–12	117	687	75
13–14	64	217	34
15–16	31	882	38
17–18	95	97	26
19–20	45	130	57
21–22	71	470	34
23–24	75	117	18
25–26	40	240–F	18
27–28	45	277	66
29–30	79	210	25
31–32	116	702	38–F
33–34	52	112	32
35–36	81	320	19
37–38	24	51	23
39–40	47	112	23
41–42	39	65	
43–44	21	118	
45–46	21	205	
47–48	16	F–100	
49–50	7	377	
51–52	10	F–192	
53–54	3	76	
55–56	20	76	
57–58	7	246	
59–60	40	58	
61–62	4	138	
63–64	3	201	
65–66	3	98	
67–68	7	176	
69–70		72	
71–72		198	
73–74		217	
75–76		140–F	
77–78		62	

a very similar procedure to that used by the Sparrow No. 4 of the earlier study. Although the two boxes were not alike the method of opening was quite the same. The Sparrow used the bill at the tenth trial and stood on the floor at the twenty-seventh. The Cowbird used her bill on the forty-sixth trial and opened the door from floor at the fiftieth, though she does not consistently use this method until eleven tests later. As compared with the English Sparrow she seems rather slower in leaving off the unessential and in learning just the thing to be done. However, it is a point worthy of note that both proceed in the same manner to reduce the necessary action to its lowest terms.

The male Pigeon opened the door first and last in the same way—striking one of the strings with his bill. There was thus no opportunity for him to simplify his action. He stood on top of the box or on the floor near the box a great deal of the time. Seeing parts of the box or the food seemed to suggest with difficulty the required actions. He early established the habit of going for a drink and sometimes opened the door on his return. His manner seemed to indicate considerable indecision, even after he had approached the box a number of times. In the fourteenth trial he struck the string three times after the door had swung open. The same thing occurred twice in the following trial, once in the twenty-fourth and the twenty-sixth, twice in the twenty-seventh, once in the twenty-eighth, and four times in the sixty-eighth. Three of the latter were rather vicious hard pulls on the string, and after starting to enter he turned and pulled the string the fourth time. Often after stopping in front of the strings he made several feints at striking them before really doing so. He seemed to see very keenly. The slightest shadow on the cloth side of the cage was enough to disturb him. He was very easily distracted, reminding one not a little of a nine months' old child, who apparently starts across the room for some object but forgets about it before it has scarcely started.

The female pigeon opened the door by simply walking up the side over the strings until some one of them was pushed in. In the fourth trial she used the bill on the top string. During the succeeding trials she used several methods—pushing in with her breast. and pulling the lower string with her bill or claw while standing on the floor. Not until the thirty-first trial did she use one method exclusively. This consisted in pulling down on the lower string with one foot. As with the male Pigeon, many trials show several approaches to the strings before the door is opened.

An attempt was made with the female English Sparrow whose wildness had caused me to discontinue the series with the reversed maze. She was so wild that but eight trials were made, she being successful only on the second and fourth trials. However, in the fifth and succeeding trials it was clear that she tried repeatedly to seize the strings with her claws without alighting or even touching the box. But before quite reaching them she hovered and flew away. It was probable, judging from my experience with others of her kind, that she had already selected in spite of her fear, or it may be because of it, the essential part of the apparatus. I say because of fear, for it may very well be that a reasonable amount of fear and caution may act as a stimulus to a more rapid singling out of just the required things to do, and the parts to be worked upon.

While making the tests, the results of which are recorded in an earlier paper, a Red-headed Woodpecker came in through the open window and was caught. A short series of experiments was made with the food-box, figured (9, p. 319); also several unsuccessful attempts were made to get him through the same maze as that used with the English Sparrows. He showed very little caution in his first approach to strange objects placed in the cage. The food-box he opened very readily the first time. He did this by pecking the latch; and as we all know this is, *par excellence*, his one natural method of getting his food. But succeeding trials showed lack of ability to profit by experience. This in spite of the fact that apple and other food more to his liking than that used for the English Sparrows was placed on the inside. Yet most amusing and instructive of all was the way he pounded the bottom, but chiefly the wire mesh, of the maze with his bill. It is true that the maze was a little small for him, particularly when he attempted to turn round; but instead of trying to walk through he spent the greater part of his time in pecking at the sides and bottom of the maze. Frequently he thrust his long tongue, peculiar to his kind, through the wire mesh. These results, though few and negative for the most part, serve to re-emphasize the place which very often the natural instinctive activity of the animal should be given in our efforts to study its intelligence.

These results also served to suggest for solution a question concerning the learning act in the individual animal. As indicated above, this Woodpecker opened the door very easily on the first trial; so also did the Vesper Sparrow the first times through the maze, and the male pigeon in opening the food-box. Now the query arises whether greater difficulty and therefore more vigorous activity on the part of the animal in the initial trials of any series may naturally be expected to lead to more rapid progress in the later ones. The greater activity would give through the back-stroke impulses a richer experience. All the movements except the one bringing the right result may be looked upon as having a slightly disagreeable affective tone; at least, the movement opening the door or bringing the correct adjustment of the organism is affectively colored in an agreeable way. The trouble which the Cowbird had with the Simple Maze, and the fear and caution which she had in consequence of this, may have made her react in this particular experiment in a way similar to the habitual way of the English Sparrow. With the food-box, designs, colors, and forms she was much more at ease. These facts are far from conclusive and all that is attempted is again to call attention to a very desirable further analysis of the learning act, especially as it is modified by those mental states which are more predominantly affective and motor.

There is some ground in what has just been said for thinking that the first half of the proverb, "No man learneth but by pain and shame," may very well be true of animal learning as well. To be sure, the difficulty, activity, or emotional condition must not be too extreme. "Soon learnt, soon forgotten," and "One learns by falling (failing)" may be found to be even more applicable. The above suggestions gain added weight from the fact that Professors Jennings ('04 and '05) and Pillsbury ('06) have recently so extended the trial and error method as to make it constitute, according to the former, the essential principle of regulation, chiefly of behavior, in the inorganic as well as the organic, according to the latter, as the essential factor in evolution, even including the social life of man. As further proof that this same method may be found to apply to the instinctive activity of higher invertebrates, the reader is referred to the abstract of the writer's work with Spiders (10, pp. 44, 45). The original paper will appear in the next number of this *Journal*.

The preceding may be of some help in interpreting the failures recorded in the above table, particularly those against the male Pigeon. Some of these fall late in the series. Such is not the case for the Cowbird except during the first few tests, and the female Pigeon failed but once after she was well along in the series. The time measure for the male Pigeon is irregular and very slowly drops toward the minimum, indicating that learning is rather a slow and uncertain process with him. The Cowbird is considerably better, but not quite so rapid and well sustained as the female Pigeon; while if we compare the time for the English Sparrows it will be seen that in general they are very similar. While the times for the English Sparrows show a better ability to learn than even the female Pigeon and the Cowbird, yet the variations are not so great but that they may be cancelled by individual differences. It is quite obvious that the cases tried are too few to permit of any elimination of these differences.

MEMORY TESTS.

After an interval of thirty days these same three birds were tried again with the same box. As before, with the simple maze, there were no intervening tests. In general the same differences obtain in these memory tests as in the initial series. There is not much difference between the Cowbird and the

TABLE V.

Memory Tests with Food-box. Thirty Days Interval.

No. of Trial.	COWBIRD.	PIGEON, Female.	PIGEON, Male.
	Time.	Time.	Time.
1–2	65	102	36
3–4	12	320	27
5–6	36	20	14
7–8	46	29	15
9–10	8	38	19
11–12	12	23	87
13–14	12	19	40
15–16	39	30	32

female Pigeon. The male Pigeon is not long in returning to his former record. It should be added, however, that I had learned by this time to take extra precautions to be sure that the male Pigeon was really hungry. There were some indications that the over-night interval did not make him as hungry as the other birds, especially the English Sparrows. If there is really a difference of this kind here, it is very important, and should be taken account of in later experiments.

Both the Cowbird and the female Pigeon lapsed to their earlier way of opening the box after this interval of thirty days. It will be recalled by those who read the results of my first study that a change in the fastening caused the English Sparrow to revert to his old habit. But it is interesting to note that his following of the old habit is of short duration, lasting only during the first few trials. The male Pigeon's method, as pointed out above, was as simple from the first as it could be. But in this memory test he must approach the box as many as four times before acting. In the following trials there is less of this.

One hundred and twenty days later for the Cowbird and one hundred and forty days for the male Pigeon a second memory series was obtained. It is evident from Table VI that there is greater loss through this longer lapse of time and the same difference between the two birds is more marked than in the other memory series.

After the thirteenth test in this series with the Pigeon, the strings which opened the door were placed to the left of the door, the original strings being left on the right, but which it now did no good to pull. The food was taken from immediately back of the old strings and placed in the same position with reference to the new ones. This change caused him some trouble. Most of the times are long and he shows a much less rapid reduction of useless efforts than the female Sparrow (9, pp. 22 and 23), with which a very similar test was made. He

Table VI.

Memory Tests with Food-box. Second Series. Intervals 120 Days for Cowbird and 140 for Pigeon.

No. of Trials.	Cowbird.	Pigeon, Male.
	Time.	Time.
1–2	132	1800–F
3–4	37	355
5–6	54	24
7–8	34	80
9–10	15	132
11–12		33
–13		55

is also slower and more irregular in reducing the time to a minimum.

Tests with Designs.

These were made only with the Cowbird. The test was an exact repetition of the one made earlier with the female English Sparrow. Glasses covered with gray paper were surmounted by cards carrying designs. The food was first placed in the glass carrying the card with three horizontal black bars, and alongside of this was placed a similar glass carrying a blank card. Next the food was placed in the glass with the card carrying a black diamond, and used as before along with the glass carrying a blank card. In the third series the horizontal bars and the diamond were used, the food being in the glass bearing the diamond-marked card.

The place of the food-glass was irregularly shifted from right to left and *vice versa* while the bird was behind me. Twenty tests were made at each sitting. If the bird came to the wrong glass she was allowed to feed a little out of the right one. The table below gives the correct number of choices out of twenty trials for both the Cowbird and the English Sparrow, the results for the latter being taken from my earlier work (9, p. 345).

Food in Glass with Horizontal Lines.

Cowbird,	11	16	17	19	19	19	18	18	19	19	19	
English Sparrow,	14	13	15	14	15	16	16	20	20	18	17	20

Food in Diamond-Marked Glass.

Cowbird,	16	17	14	17	19	20
English Sparrow,	18	14	12	18	18	20

Food in Diamond-Marked Glass to be Distinguished from one with Horizontal Lines.

Cowbird,	1	4	6	7	10	12	12	16	17	19
English Sparrow,	11	14	12	18	18	20				

It should be noted that the Sparrow made alternate choices from opposite ends of the cage while the Cowbird flew always from the same perch. The Sparrow usually alighted directly on the edge of the glass of her choice but the Cowbird always flew to the floor and then walked slowly to the glass of her choice. Her vacillation or indecision was very noticeable at times. The first and second series show little if any difference in the rate of learning or ability to discriminate. However in the last series where somewhat nicer discrimination is called for and an old habit must be inhibited the Cowbird seems much the slower. Some may ask if it is not possible to find designs that are not so foreign to the animal's experience. My answer would be that such may be quite possible. The reason for using these was the fact that Dr. Kinnaman used them with his monkeys and obtained negative results with them. By using the same apparatus and methods of experimentation as nearly as may be is the only way to obtain results which will admit of close and exact comparison. Both the Sparrow and the Cowbird learn to discriminate these designs from no design and from each other and it was that the comparison might be made as exact as possible that these designs were used.

COLOR DISCRIMINATION.

Six glasses were covered with the Bradley colored papers, a dark gray, a light gray, a bright yellow, a dark blue, a light green, and a dark red. Samples of the papers used with the Sparrow were used to match from. These glasses were placed in holes in a board. The same programme which determined the place of the glass for the Sparrow was used here. Here also twenty tests were made in each successive series. The following table gives the number of correct choices for each series with the results for the Sparrow again brought forward for comparison.

FOOD IN BLUE.

Cowbird,	5	9	11	12	12
English Sparrow,	12	14	13	12	19

FOOD IN YELLOW.

Cowbird,	4	4	9	11	10	11	15	18	17	18
English Sparrow,	0	5	1	5	8	16	11	13	18	19

FOOD IN RED.

Cowbird,	8	6	14	16	17	20
English Sparrow,	2	1	9	15	14	20

FOOD IN GREEN.

Cowbird,	6	10	16	18	17
English Sparrow,	9	17	15	14	18

Both birds learn to discriminate between the different colors at almost an equal rate. The first color used, the blue, showing the widest difference in favor of the Sparrow. Again, it should be added the latter's approach was much more direct and her choice seemed more decisive. The closest attention on my part was necessary to see that the Cowbird did not peek into the glass next to the food glass before committing herself to a final choice. She did not manifest the color preferences which the Sparrow was found to have. The latter was at first disposed to avoid red, yellow and green.

The few tests which I was able to make with the forms indicate that the Cowbird was learning to distinguish the triangular from all the others. This the Sparrow could or did not do. Yet there was one important difference which crept into my way of trying the experiment with the Cowbird. The box containing the food was shifted each time, it is true, but the triangular box always presented the same face to her. With the Sparrow there was a constant shifting of faces, and it is possible that this makes it too difficult. Yet it will be most interesting to determine if the Cowbird in her detection of other nests, and within limits of those best for the preservation of her species, gets any help, instinctive or otherwise, in the correct distinguishing of these forms. Some tests with the same male monkey which Dr. Kinnaman experimented upon, indicate that he has considerably greater difficulty in distinguishing these forms than those which Dr. Kinnaman used. The latter, it should be stated, were not so free from differences in size.

AFFECTIVE AND VOLITIONAL STATES.

Since this work was begun attention has been given to the emotional and volitional reactions of the birds dealt with. When confined in the small cages the English Sparrows were most frightened by my near approach. They were as cautious as the Cowbird, perhaps more so, while the Pigeons and the Vesper Sparrow showed little of caution. The former sustained their reputation for timidity while the latter seemed so fresh from wild nature itself that it could not be otherwise than trustful. The female English Sparrow, as we have seen, was too fearful to open the food-box in a reasonable length of time; yet when the door to this box was open, and the male Pigeon inside, the same Sparrow entered and re-entered, although the Pigeon had already driven her out.

When conditions were reversed the Pigeon sometimes failed to enter when the Sparrow held possession. This characteristic boldness of the English Sparrow is seen in the way it frequently despoils the Robin of the worm the latter has just found. At most any time in late summer on the University lawn one may see a Sparrow alight some six or eight feet away, wait until the Robin has found a tempting morsel, then rush in and relieve him of it. Even the Cowbird, perhaps, accomplishes the laying of her egg in some other bird's nest rather through stealth than by driving the rightful owner from the nest. It seems to me that this stealthy manner was evident many times during the experiments with her. When confined in small cages with English Sparrows she was able to lord it over only the sick ones. She seemed to keep her weather eye open, and especially just after alighting would turn her head half way over so that she might take in the whole expanse of the sky. Rouse (11, pp. 512 and 513) in his study of Respiration and Emotion in Pigeons, finds that the breathing previous to habituation is profoundly influenced by sounds, mechanical jars, and, perhaps, odor, jars particularly causing abrupt inhibitions and irregularities. Stimulation by light caused much less disturbance than mechanical stimuli. It is probable that *agreeable feelings* are accompanied by respiratory quickening (and perhaps by shallowing and irregularity). After hearing a report of the present paper (10, p. 45), Mr. Rouse tells me that his later results on the intelligence of the Pigeon point to the same general differences in favor of the English Sparrow.

The English Sparrow is more active than any of the other birds. This with his boldness and caution makes him appear, at least, to have a goodly amount of curiosity. This, according to many writers on Psychology, is the best possible basis for a rapid development of the learning process. I refer here especially to lectures by Prof. Wm. H. Burnham, of Clark University, on such instincts as those of Workmanship, the Instinct for Further Cognition, etc.

Kinnaman (5, pp. 195 and 196) says of the individual differences between his two monkeys: "In their reaction to the locks, the male moved more rapidly about the boxes. He tried more persistently and gave up only after many fruitless efforts. . . . She always seemed stupid in the beginning, but often came suddenly to the idea, and finally, if it was an easy thing, learned it more quickly than he. . . . In all of the very difficult things he appeared to be superior, but for the easy things she was superior. If we turn to the association with forms the reactions were very much alike. . . . The female learned the colors more readily than the male." The author suggests that the male was too rapid and nervous for the best results in these last two tests; so also for the lower numbers of the number tests, but he continued the associations up to six, while she stopped at three.

Watson (14, p. 47) has very well emphasized the necessity of keeping the kind of test well in mind if we would distinguish between mental accomplishments of animals. He says: "Considering first a problem where mere activity is at a premium for its solution, we find that the adult rat consumes more time for a first solution than does a young rat, but that for a second solution this difference in favor of the immature rat is not so marked. . . . Considering next a problem not so conditioned upon activity—such as the test with Box II—we find that not only does the adult rat show a smaller record for the second success, but that even the time for the *first* success is *much* less than is the time for the first success of any group of the young rats." There are many more useless movements made by the young rats. Small (13, p. 159) also makes superior vitality and activity explain the advantageous showing which his young rats made. The general impression which one gets from seeing my birds go through the tests (my attempts to actually measure the amount of their daily activity not having progressed far enough yet to allow of any definite results) is that the English Sparrow is most active of any, the pigeons being least so.

The Cowbird imitates the Sparrow in following her to the food-box. In turn it was thought that the Sparrow showed some signs of reacting in the same manner as the Cowbird toward the fastenings of the food-box. About the first of August, in Illinois, I have seen large flocks of these Cowbirds, many of which were apparently young ones. At this season they seem to feel that they belong together as the young of English Sparrows do. The young of both no doubt are better able to survive by association with the old and each other.

All the birds are able to confine their attention to a particular part of the box. This implies control of an inhibitory as well as a positive kind. Roughly speaking, the Pigeon and Vesper Sparrow showed least of this. They were not tried with the same apparatus, so that the above statement means so far as their respective tests are concerned and as contrasted with the other birds, Cowbird and English Sparrow. The Sparrow is no better in many of the tests, so far as this control is concerned, than the Cowbird. In the breaking up of an old habit where it antagonizes the new, and in the early reduction of a habit to its lowest terms, the English Sparrow is perhaps a little better than any of the others. The writer hopes that the present paper may approach more nearly to the standard recently set forth by Prof. Mills (7, p. 751), namely, that animals be tested in such manner as will bring out their powers of inhibition and control.

However true the above differences may be, or may be found to be on further experimentation, the writer does not hold that the facts set forth in the present paper are at all sufficient in number to permit of any safe generalizations as to the superiority, in a mental way, of any one species of bird over another.

A summary of the results obtained and conclusions arrived at may be stated as follows:

1. The Vesper Sparrow, Cowbird, and four English Sparrows, learn the simple maze, a plan of which is shown on page 253. They all have it pretty well learned by the 15th-20th trial. After qualifying conditions are allowed for, there is perhaps little superiority of the English Sparrows over the Cowbird. The Vesper Sparrow seems to be the slowest in learning, a fact which is further strengthened by her changing fifteen times out of thirty from the long to the short way.

2. The memory tests with this same maze thirty days later show a surprising memory on the part of these birds. The Cowbird is best here. Her average for ten trials quite equalling that of the last ten of the initial series. The Vesper Sparrow and the female English Sparrow do not do quite so well. The latter was too young and too easily frightened to be a good subject.

3. The results with the food-box, see Fig. 2, page 257, which was used with the Cowbird and a male and female pigeon, give evidence of ability to rapidly profit by experience on the part of these birds. There are more failures later in the series by the Pigeon, especially for the male, than for the Cowbird. If we compare these birds with the English Sparrow, the latter were tried with a different box but it was not an essentially different fastening, there would seem to be a more rapid and consistent reduction by the English Sparrows of the time and efforts put forth. If we include the further simplification of the act of opening the door and its saving in effort, the English Sparrow is perhaps superior.

4. Memory tests after thirty days with these same three birds and the same box, no other tests being made in the meantime, show, in addition to the experiments on memory described above, that the Pigeons as well have a fairly good memory. This lapse of time has the interesting result of causing a reversion to a habit older than the one used when the former series ended. An interval of one hundred and twenty days for the Cowbird and one hundred and forty for the male Pigeon showed that much more was forgotten than during the former shorter interval.

5. The Cowbird learns to distinguish between the different designs—the three horizontal black lines on one card to be distinguished from a blank card, a card marked with a black diamond, from a blank card, and the two marked cards. There is not much difference here between the Cowbird and the English

Sparrow. The latter unlearns the old habit and therefore learns the new one earlier in the third series which, of course, is the most difficult.

6. In learning to distinguish between the colors the female English Sparrow does better with the first color tried—blue. After that the difference is not so great.

7. The tests with forms had to be left unfinished; but the Cowbird showed that she was learning to distinguish the triangle. The sparrow gave no evidence of this. Whether or not this is a real difference between the birds or is due to a different method of trying the experiments, must be left for further investigation to decide. Experiments with Jack, the same male monkey which Dr. Kinnaman experimented with, and my form boxes show that he has considerably more difficulty with these from which differences in size have been eliminated, than with those of Dr. Kinnaman.

8. The English Sparrow when confined in the same cage with the other birds has shown itself more capable of fear, courage or boldness, caution, and independent action. It was more of a leader, more persistent and more active. The Cowbird was almost as wary as the Sparrow yet at other times she was not so bold. The Pigeon's popular reputation for timidity and keen sense of vision was well borne out by my observations and experiments. The value to the animal of these differences, some of them marked, in emotional and volitional states is apt to be overlooked or, if noted, underestimated.

9. All, of course, show a distraction of attention as is quite to be expected from wild birds experimented on in captivity. This distraction or inability to keep the attention on one thing was (barring the wildness of the young female English Sparrow) most noticeable in the Pigeons, Vesper Sparrow, and Cowbird, and least in English Sparrows.

It is a pleasure to me to again acknowledge helpful suggestions from Professor E. C. Sanford; also to Mr. Toshi-yasu Kuma for making for me most of the Form tests on Jack, the monkey, and to Mr. Burton N. Gates, a fellow student, for the female pigeon used in some of my experiments.

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HYPNAGOGIC IMAGES AND BI-VISION IN EARLY CHILDHOOD. A NOTE.

By ALEXANDER F. AND ISABEL C. CHAMBERLAIN.

The subject of this note is the daughter of the authors, aged, at the time of the observation, 4 years and 4 months. On November 25, 1905, she said to her mother:

"Mama, do you see pictures, when you shut your eyes when it's dark?"

Her mother replied, "Yes, sometimes. Do you?"

Whereupon the conversation continued:

R. Yes, I see all kinds of funny men and animals.

M What else do you see?

R. Sometimes, when I shut my eyes, I seem to go right down through the floor.

M. What do you do down there?

R. O, I see all sorts of funny things.

This conversation took place in the day-time. The next night, after she had been in bed a few minutes, Ruth called her mother, and said:

"Mama, when I shut my eyes and go down through the floor (just make-believe, you know, not really), why, sometimes I go into water. It's just make-believe water, is n't it? There is n't water really under the floor, is there?"

To this her mother replied: "Oh, no, that is just a kind of dream where you see the funny things and have funny things happen to you."

And Ruth continued: "Yes, I suppose, it must be dreamland where you go when you shut your eyes."

On December 4, 1905, her father asked her some questions concerning these "pictures." In her answers she said that she "saw it all crowded up with funny people and things," and that "they keep changing to something alike."

This discovery of "hypnagogic images" by so young a child is, perhaps, worth placing on record here. The thing had never been discussed or ever mentioned by her parents or by any one of her environment or acquaintance, and her announcement of seeing pictures with her eyes shut was entirely spontaneous and not led up to or suggested by word or action of other persons. The "going down through the floor," the "water," the "crowding up," and the "keep changing to something alike," are interesting items in the description of the phenomena in question.

Another observation, relating to bi-vision, may also be recorded here. On November 14, 1905, Ruth said to her father, "When I look at my nose, I see two tathers (fathers)."

It would seem as if some of the psychological and physiological phenomena now on record only for a much older period of life, not only occur in early childhood, but are perceived by children and may possibly exert some influence upon the development of the individual.

THE ELECTRICAL SUPPLY IN THE NEW PSYCHOLOGICAL LABORATORY AT THE LELAND STANFORD, JR., UNIVERSITY.[1]

By LILLIEN J. MARTIN.

The Electrical Supply. During the construction of the laboratory it was wired for three different electric currents:

1. *The alternating current*, which is supplied to each room and closet for illuminating purposes, for making experiments with light, and for use with motors constructed for this particular current. (1) This current might also be employed with small direct current motors, but the abundant supply of more suitable currents makes its use for this purpose unnecessary in this laboratory. (2) Aside from the four clusters of bulbs for illuminating purposes, the lecture room is supplied with this current at five other points, making it possible conveniently to shift the stereoptical and other optical instruments to the place desired. The alternating current, through the use of a transformer, runs an induction motor which drives two direct current generators, and these furnish the direct current.

2. *The direct current*. The generators for producing this current are placed in the basement of the building and also supply electric power to the Physics Laboratory. The switches, etc., used in setting this machinery in motion are so easily manipulated that even an inexperienced person, as regards the handling of such machinery, is easily taught how to turn on and shut off the current as may be desired. There is one current with two outlets of 55 volts each supplied to each room of the psychological laboratory. In the lecture room there are four such currents placed in different parts of the room. Each current is admitted and shut off by a Lang switch

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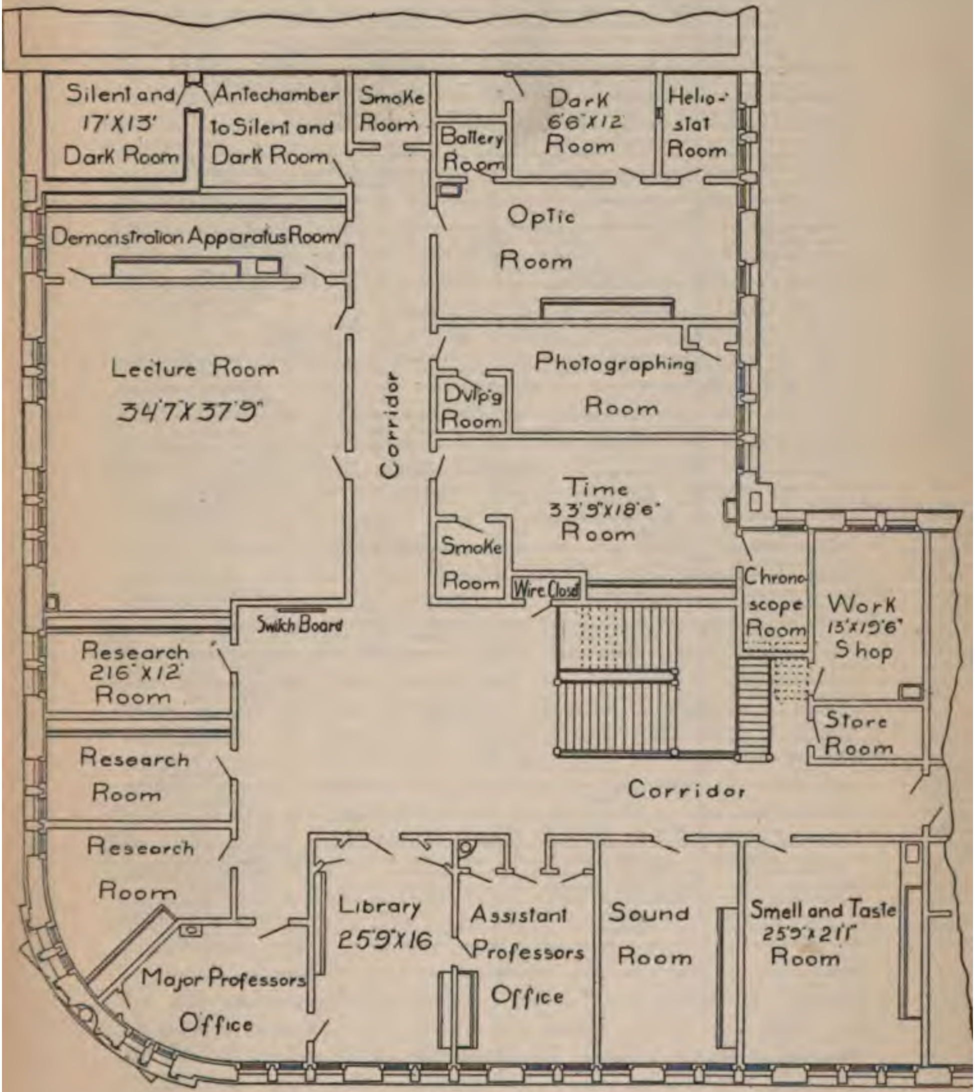
[1] PLAN OF THE PSYCHOLOGICAL LABORATORY.

Special rooms have been set apart for time, optical, sound and smell experiments only. The apparatus for other classes of experiments is placed in the cases in the research rooms and can be used in those or other rooms. Much of the apparatus used by the beginners in laboratory work has been duplicated and is stored in cases in the hall. All of the apparatus used for a given experiment is set out on a large table, which has been placed there for the purpose, and from this it is distributed. According to the original plan the smoke rooms, used for smoking drums, were to have been provided with flues for carrying away the superfluous smoke, but as yet these have not been put in.



PLAN OF LABORATORY.

which for purposes of protection is supplied with a fuse. The switch is mounted on a square board (see Fig. 2, B) on which the positive and negative poles are marked, and this is placed at the side of the room storage battery switchboard. When a suitable form as regards range of measurement and size of apparatus can be found, a rheostat for controlling the strength of the current is to be permanently mounted on each of these boards. At present the current is roughly controlled by a set of Scripture lamp-batteries with darkened bulbs, and more finely by connecting through plugs attached to the board one of the Rosenbach's rheostats made by Spindler and Hoyer of Göttingen (see Fig. 2, C). This current is for use when a current above 12 volts is required. It also supplies the current with which the storage batteries are charged.

3. *Storage and other battery currents.* Expense, awkwardness of manipulation, inconstancy and irregularity make the two previous currents unsatisfactory for much experimental work in the psychological laboratory. The laboratory has, therefore, been supplied with various forms of battery currents, as those generated by the storage batteries made by the Electric Storage Battery Co., by gravity batteries, by the Edison-Lalande batteries, by dry batteries, etc. The batteries are placed in the battery room, and through connection with wires running from this room to the general switchboard (see Fig. 1) the current or currents desired can be obtained in each of the rooms.

The general switchboard (made by the Drendell Switchboard Co., of San Francisco) also makes it possible to connect two or more rooms in circuit. For the ordinary purposes of the laboratory the storage battery is employed and the switchboard has been especially constructed with a view to its use, though any of the other batteries can be substituted for it should it be desired. The general switchboard is enclosed in a case having glass doors and is fixed to the wall in the hall, but for convenience of repair it has been so constructed that it can be reached from behind through doors in the wall in the lecture-room. The board is supplied with three storage battery currents, 1, 2, 3, having a voltage of 2, 6 and 12 volts respectively. The amperage of 1 and 2 batteries is 10 and of 3, 8 amperes. Current 1 is supplied by one element of the "Chloride Accumulators," manufactured by the Electric Storage Battery Co., of the type "E" with 5 plates, 7¾ in.x7¾ in.; current 2 by connecting in series three of the same elements; and current 3 by similarly connecting 6 of the elements of the type "D" with 7 plates 6 in. x 6 in. By turning the arms of the switches V and A which connect the voltmeter a and ammeters b, b, b, with the various batteries on 1, 2 or 3, it is possible to determine the strength of the charge of that particular battery at

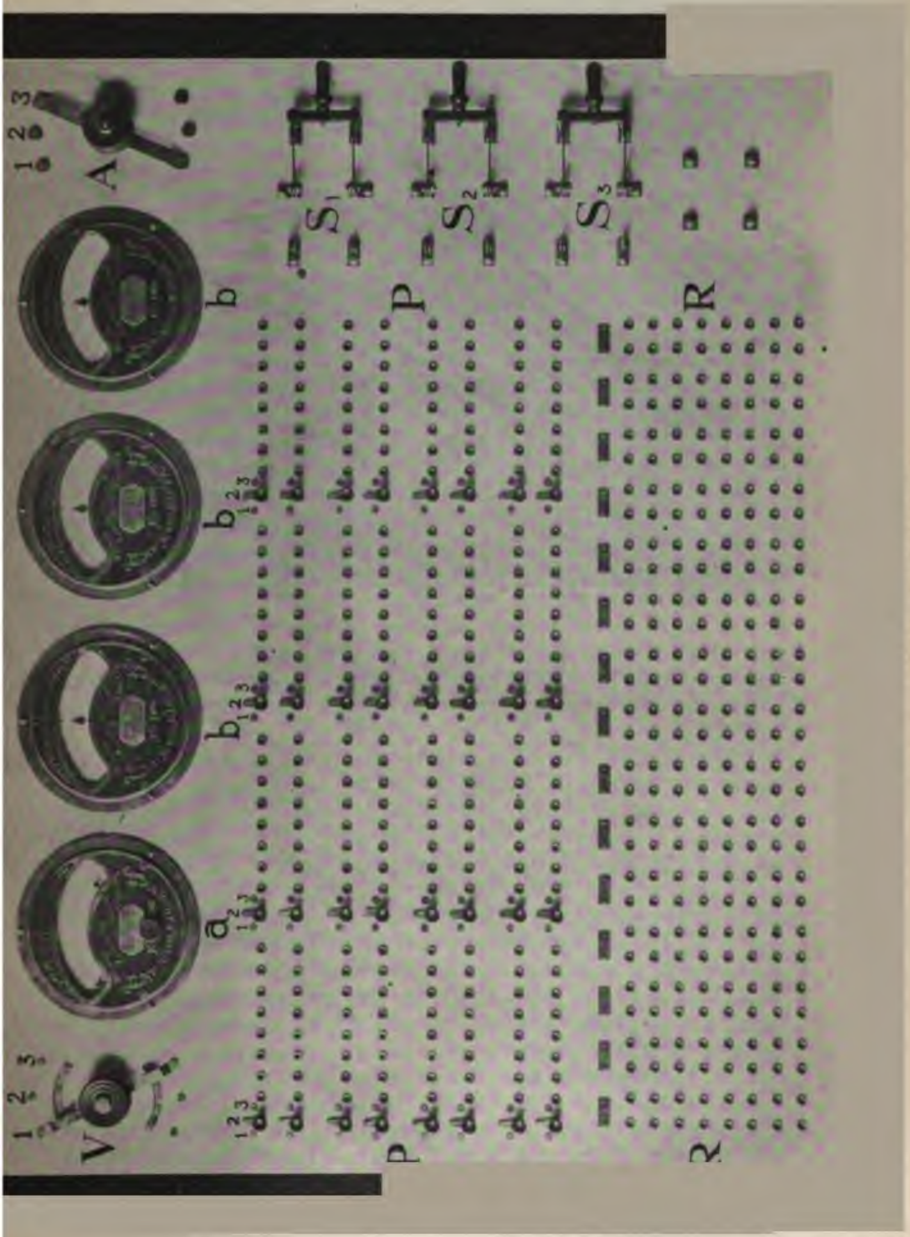


FIG. 1. LARGE SWITCHBOARD.

the moment, and should any battery need restoring to its normal strength, in order to avoid the injury that comes from allowing the battery to stand incompletely charged, this can be done by turning the arm of the switch A, on 1, 2 or 3, and reversing the switch S_1, S_2, S_3 which supplies the direct current to that particular battery. As regards the completeness of the charge in the batteries, the voltmeter acts as a danger signal. The current must not be allowed to fall below a certain voltage before being restored. In the renewing of the charge the ammeters show when the required charge has been reached. The voltmeter and ammeters on the switchboard give, of course, only the voltage and amperage of the current at the board. A portable voltmeter and ammeter is used for getting the current strength after it has reached the rooms and for finding the resistance offered by a given piece of apparatus. The arms of the small switches on the board in connection with one set of plug holes PP bring the particular currents desired to these different holes, and by connecting these holes through plugs with one or more of the pairs of holes RR of the sets of eight pairs of holes which belong to each room, it is possible to obtain eight currents in each room having a voltage of 2, 6 or 12 volts and an amperage of 8 or 10 amperes. Moreover, by proper plugging on the same board we may connect in circuit any or all of these rooms together in any way desired. Each room has a small switchboard (see Fig. 2, A) with holes corresponding to those on the large board for this particular room, so that no confusion regarding the particular current connected may arise.

The room switchboards make it possible, by connecting their terminals parallel or in series, and by introducing the Rosenbach rheostat, to obtain any voltage from the storage battery between a fraction of a volt and 20 volts. The current can also be reversed through using the switchboards. Should it be desirable to connect different rooms in circuit in using the direct current, the direct may be substituted on the general switchboard for the storage battery current. In that case, however, if the current was sent by but one wire to a room a resistance would have to be introduced, as the individual wires are too small for carrying the full current of 55 volts. Should it be desired for any purpose to put the full current of 55 volts in circuit, this could be done by dividing it by using the 8 pairs of wires belonging to each room. In that case, of course, no resistance would have to be inserted at the switchboard. As regards cost of the apparatus for supplying the storage battery current to the laboratory, it may be said that the switchboards and batteries and the putting of them in position cost approximately $1,500.

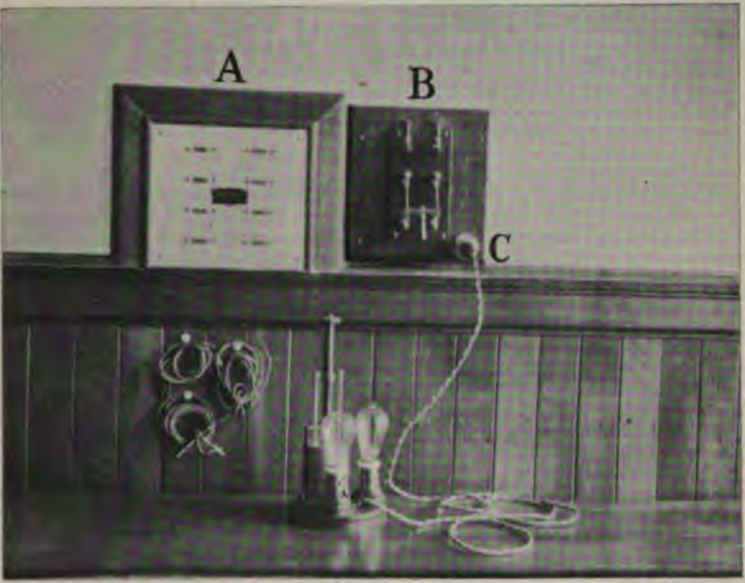


FIG. 2.

In what has been said an effort has been made to show that ten currents, varying in any strength desired from a fraction of a volt to 55 volts, are available at any time in each room of this laboratory. Indeed, we may say not 55 but 110 volts, for by a slight change in the connections with the generator this voltage can be obtained. It is evident that we have here not only the amount of electric current necessary to run the large motor of a workshop and the smaller motors used in color mixing and rotating kymographs, for holding the shutter of a tachistoscope, for the ringing of the signal bells, etc., etc., but a current that is sufficiently constant and regular to give accurate results when employed in the Hipp chronoscope and in time markers.

PSYCHOLOGICAL LITERATURE.

The Dissociation of a Personality, by MORTON PRINCE, M. D. Longmans, Green & Co., New York, 1906. pp. 569. $2.80 net.

The study of abnormal personality has been enriched by the careful analysis of a truly remarkable case. The case of Miss Beauchamp is noteworthy for the treatment to which Dr. Prince subjected the unfoldment of the distorted selves, quite as much as for any spontaneous evolution of the conflicting personalities. For just as the trance-states that flourished under the belief in spiritualism were cultivated by the reception which they met, so will the attitude of the physician towards hysterical dissociation play a part in the maturing of the disintegrated phases of personality. It is through his command of suggestion thus acquired that Dr. Prince was able eventually to discover and restore the original self to a possession of a fairly consistent personality. In this respect the issue is parallel to that which Dr. Sidis was able to establish in the case of Mr. Hanna. Yet the two cases fall in different classes and present quite opposite characteristics. The case of Mr. Hanna belongs to cases of loss of personality through degradation of functions from higher to lower ranges;—what Professor James in the case of Mr. Bourne calls a shrunken, amnesic, abstract of the original self. The case of Miss Beauchamp is one of a warped growth of a personality in the making; and its features accordingly reflect the stress and strain of late adolescence. The unfoldments of the plot centre about the personal emotions, largely infused with moral judgments of *licet* and *non-licet* and with conflicts between ideals and the practical possibilities of their realization. Yet underlying all this must be a more tangible substratum of functional disorder, that induces differences of assimilation and response, of taste and disposition, and deprives each phase of the personality from more than an uncertain participation in the life of the other.

The case is in itself so noteworthy that an outline of its development will be worth reproducing.[1] The subject, "Miss Beauchamp," was about 23 years old when she came under Dr. Prince's care. Her girlhood presented no unusual complications and yet was such as to intensify her natural emotional susceptibilities. There were a few vagaries of action and pronounced eccentricities of character, but nothing that prevented her from being highly regarded by her friends, from whom indeed she was able to conceal the fluctuations of personality that overtook her. At the time of seeking Dr. Prince's care she was a student at college, and of her condition during the next few years, the period of her greatest instability, Dr. Prince writes: "She may change her personality from time to time, often from hour to hour, and with each change her character becomes transformed and her memories altered. In addition to the Real, Original or Normal Self, the Self that was born and which she was intended by nature to be, she may be any one of three different persons. I say three different persons because, although making use of the same body, each, nevertheless, has a distinctly different character; a difference manifested by different trains of thought, by different views, beliefs, ideals, and temperaments, and by different acquisitions, tastes, habits, experiences, and memories. Each varies in these respects from the other two, and from the original Miss Beauchamp. Two of these personalities have no knowledge of each other or of the third, excepting such information as may be obtained by inference or second-hand, so that in the memory of each of these two there are blanks which correspond to the time when the others are in the flesh. Of a sudden one or the other wakes up to find herself, she knows not where, and ignorant of what she has said or done the moment before. Only one of the three has knowledge of the lives of the others, and this one presents such a bizarre character, so far removed from the others in individuality, that the transformation from one of the other personalities to herself is one of the most striking and dramatic features of the case. The personalities come and go in kaleidoscopic succession, many changes often being made in the course of twenty-four hours. And so it happens that Miss Beauchamp, if I may use the name to designate several distinct people, at one moment says and does and plans and arranges something to which a short time before she most strongly objected, indulges tastes which a moment before would have been abhorrent to her ideals, and undoes or destroys what she had just laboriously planned and arranged."

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[1] This is taken in large part from the account given by the present reviewer in a volume on *The Subconscious*, now in press.

Of the several variants of Miss B. the most influential took the name of Sally; this personality, in the period of its greatest dominance, may be said to embody the opposition,—the organization of forces that thwarts the supremacy of the real Miss B., and the destruction of which, as the sequel proved, was essential to the restoration of the former. The hostility of Sally to whichever personality was in command may first be illustrated. Miss B., who has an abhorrence of insects and reptiles, finds a box neatly tied, from which, as she opens it, six spiders run out. Sally is subconsciously present to witness the effect of her practical joke, and thus describes the incident: "She screamed when she opened the box, and they ran out all over the room." Special expeditions into the country were made to secure spiders and snakes and toads,—walks that were altogether too taxing for Miss B.'s strength. Sally never felt fatigue; yet naturally their common body showed the effect of such a strain. On one such occasion Sally walked to a suburban town, in which she came to herself as Miss B., utterly stranded and exhausted. To torment Miss B., Sally would unravel the worsted work upon which the former was engaged, and when she permitted its completion, "pulled the whole of it to pieces, and drawing out the yarn wound it round about the furniture, carrying it from picture to picture, back to the different articles of furniture, then round herself many times, then back to the furniture, finally hiding the ends somewhere in the bed. Then Sally, standing in the midst of this perfect tangle of yarn, wakened Miss Beauchamp, who came to herself in the maze. So great was the tangle that she had to cut the yarn to get out." Sally likewise invades the premises of Miss B.'s intentions and coerces her to tell nonsensical lies, and to act upon impulses which the latter entirely repudiates, or is compelled with much embarrassment to explain away. Likewise she chastens by imposing penance, wise or foolish, and generally inconvenient: Discovering that Miss B. has been careless in money matters, Sally takes charge of the purse and hides all the money, leaving only enough in sight for car-fare and the most penurious allowance.

The complexity of this relation was such as to make possible the masquerading of the one personality in the character of the other. The situation will be suggested by the following instance that occurred when Miss B. was at the hospital, and a European tour was projected: Dr. Prince was suspicious, and found the following state of affairs: "It came to light that Sally had conceived the idea that, as she herself was free from ailments, if she could impersonate Miss Beauchamp she would be considered well, and so escape from the hospital and go to Europe, as had been previously planned. So, when the night nurse looked in upon her, Sally was always found 'asleep;' the day nurse had an equally good report to make, and Miss Beauchamp was soon, in spite of my warnings, discharged 'well.' A few days after this I caught Sally just in time, on the verge of her departure for Europe, and changed her, against her will, to Miss Beauchamp, who was astounded to find herself in my office, her last recollection being her entrance into the hospital ten days previously. It was thus by a lucky chance that Sally did not go to Europe instead of Miss Beauchamp." The measure of control that Sally possesses was well illustrated in her determination at a critical juncture that Miss B. should not be awakened. "Arguments, expostulations, even threats were of no avail. She did not want to be the other one, of whom she spoke in contempt. She simply defied me to wake Miss Beauchamp, and in fact every attempt on my part was unsuccessful. Finally, we compromised; she agreed to allow Miss Beauchamp to be awakened, and I, on my part, agreed (may the ruse be pardoned) that Sally should come again when Miss Beauchamp was *well*." We begin to appreciate why, in view of the marked differences of character which Miss B. and Sally and the personality that emerged later proved to have, Dr. Prince confesses to a temptation to call his volume, "The Saint, the Woman, and the Devil."

The secret of the situation that long remained unrevealed was disentangled by the discovery that the Miss B. who applied to Dr. Prince for relief was, after all, not the original self. This discovery came about by the sudden appearance, as a consequence of hypnotic alteration, of a strange individual who went back to an experience of some six years earlier, and who interpreted the actual situation in terms of that earlier upsetting experience. This traumatic shock, which writers upon hysteria are disposed to consider as a constant factor in the attack, was in this instance of an emotional nature and presumably occasioned the eruption that led to the serious dissociation. Naturally, this awakened self, that in turn was favored by development through suggestion, was ignorant of Miss B.'s experience during the intermediate years. And to abbreviate a story that can only be understood as well as appreciated when biographically illuminated, as Dr. Prince has so ably done, it may be said that a fusion of this older self (and the extinction of the interfering Sally) with certain phases of the later counterpart actually led to a restoration of an individual who was capable of conducting the combined affairs of the Beauchamp household under a consistent regulation.

While the curtailment of this portion of the development necessarily throws the interested student upon the resources of the original, certain further illustrations of the manner of intercourse that came to be established between the several selves may be cited for their decided psychological value. It is theoretically important to note how the one personality acts as a subconscious mentor for the other. Actions that Miss B. performs in moments of distraction will accordingly be recorded in the memory of the subconscious Sally. Sally's powers in this direction are neatly shown in the following incident related in her own words: "'She' yesterday received a letter from a photographer. 'She' had it in her hand while walking down Washington Street, and then put it into 'her' pocket (side pocket of coat) where

'She' kept 'her' watch and money (banknotes). As 'She' walked along 'She' took out the money and tore it into pieces, thinking it was the letter from the photographer. 'She' threw the money into the street as 'She' said to 'herself,' 'I wish they would not write on this bond paper.'" As further proof of Sally's knowledge, she quoted the entire letter verbatim. Sally's undisguised glee in the discomfiture that Miss B. would experience upon discovering the loss of her money, discloses the nature of her animosity. Miss B. was now awakened and acknowledged that she had received such a letter, which, however, she had torn up, but that she had in her pocket two ten-dollar notes. She put her hand in her pocket and with great surprise found only the letter. The instance is the more convincing because Miss B. also professes the faculty, allied to that of "crystal vision," by which with special effort she can penetrate into the regions removed from conscious recall and see as a projected vision what her conscious memory does not reach. By such a process she was astonished to see in the glass-globe herself walking along Washington Street, putting the letter into her pocket and tearing into fragments pieces of green paper.

Not alone were the memory spheres and the dominances of the alternating personalities quite different, but what would be a symbol of an experience to the one was wholly negatived by the other,—a relation familiarly illustrated in the negative hallucinations of hypnosis and the psychic anæsthesias of hysteria. The most remarkable illustration of this contraction of the field of sensation is the following: the Miss B of this incident is the individual known as B IV,[1] the personality that reappeared after six years' sleep. While carelessly fingering a chain upon which some rings were strung, the chain broke and some of the rings were lost. Now the other Miss B (B I) in her uninformed relation to the incident became convinced that all the rings were gone, although Sally, who was well aware of the whole procedure, tried to persuade her otherwise. "'The other two rings are not lost,' said Sally, 'but I can't make her see them. I have put them on her finger, but she won't see them, Dr. Prince; and I have taken her hand and made her take hold of the rings, but she won't feel them. They are round her neck now on a ribbon. I have made her take the rings in her fingers while she is here and I am gone, and I have put them on her finger; but it is no use, she won't see them.'" When Dr. Prince awakened her as B I, he asked her to loosen her collar and showed her the two rings tied on a ribbon about her neck, but though he passed her fingers over them and clicked the two rings together, and held them before her eyes, she was unable to become aware of their existence. He pulled the ribbon hard enough to jerk her head to one side; though she felt the movement, she regarded the method by which it was accomplished as a mystery. This negative hallucination differs from others, that could readily be induced by suggestion only in the fact of its spontaneous origin in a prejudiced conviction. The will to see for this particular range of objects was in abeyance.

It is only natural, when Dr. Prince's treatment favored the development of the restored Miss B (B IV), that Sally, as representative of the thwarting influence, should transfer her animosities to the latter. Between the two there were endless bickerings, in which Sally was obliged to write her derogatory opinions, while B IV could communicate hers by speaking aloud. It is difficult to realize the antagonisms of this divided household. "There were times when IV and Sally would enter into systematic campaigns of hostilities, each determined to down the other. Then IV would gird on her armor, and set forth resolute, uncompromising, with blood in her eye, determined to suppress Sally for good and all. She would do her best to destroy everything that her enemy wrote—many a letter to me was destroyed—and to undo everything done. Whatever she discovered Sally was doing, she would reverse. If, for example, she found herself on the way to my house, she would turn about and retrace her steps, or at least would try to do so, for Sally, in her rôle as a subconsciousness, would at once make a dive for the muscular steering gear, there would be a temporary struggle with arms and legs, a sort of aboulia, and then it usually happened that Sally, victorious, would reverse the machinery and head her again for her destination. At night, too, Sally would have another turn. As fast as IV would get into bed, Sally, coming herself, would get up, and then, changing herself back to IV, the latter would find herself to her disgust out of bed again. And so it went on all night; and if IV got off without the bed and furniture being turned upside down she was lucky.

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[1] The B IV of the narrative is the later Miss Beauchamp, while B I is the individual who applied to Dr. Prince for treatment.

Dr. Prince announces a further volume, in which the more general and theoretical aspects of disturbances of personality will be set forth. The psychologist's interests are naturally centered about such an interpretation, particularly as no other field of experience is able to supply a like insight into the complications of a normal self. It is upon such interference with the normal development of an integrated individual that psychology depends to point the way to the analysis of the normal development. In this light the story of Miss Beauchamp is a peculiarly important contribution. The very detail of its record serves to include and emphasize points of theoretical interest; for such fullness of record and psychological insight into the interpretation of the phenomena presented, Dr. Prince is entitled to the hearty appreciation of the psychological student. JOSEPH JASTROW.

The Secret of the Totem, by ANDREW LANG. Longmans, Green & Co.' New York, 1905. pp. x., 215.

This work is one of those lightly written and acutely reasoned anthropological essays, part constructive and part controversial, of which we have now had a round half-dozen from the pen of the gifted author. In some sense, it is the sequel of *Social Origins and Primal Law*, published in 1902 by Messrs. Lang and Atkinson; but it may also lay claim to rank as a novel and independent work. It contains, for the first time fully wrought out, the writer's theory of the origin of totemism. It will, perhaps, be most useful if we here sketch the main outlines of this theory, avoiding both the controversial infusions and the question of prior right on the part of other investigators to this or that feature of the whole case.

Mr. Lang starts out, not from the communal horde, but from a social status in which men were forced by economic conditions to live in small separate groups. He assumes, further, that the members of these groups were animated by the fundamental emotions of love, hate, jealousy, maternal affection, and so forth, so that there must very soon have arisen, within the groups, distinction of persons and certain practical restraints upon amatory intercourse. He inclines to accept Darwin's hypothesis of a single, strong male living with and jealously guarding several 'wives;' from such a 'family' the sons, as they grew up, would naturally be expelled, and would thus be forced to seek their own 'wives' as best they might from other similar families. Such a society would necessarily, for the younger male members, be exogamous in practice.

However this may be, we set out from small, scattered groups of men and women. These groups now, in some way or other—in what way is quite inessential to the author's theory—obtained animal names. So much is known fact. The theory offered as to the origin of these names is that they were bestowed from without, groups being named by other groups for the sake of practical differentiation. "I cannot prove, of course, that the process of adopting a name from without occurred among prehistoric men, but I have demonstrated that, among all sorts and conditions of men in our experience, the process is a *vera causa*." Mr. Lang, indeed, makes it strikingly plausible, even probable, that the group-sobriquets, although they might be of the kind that we should term nicknames, would be accepted by those upon whom they were bestowed, and also that they would be likely, in a large number of instances, to take the form of animal names.

However *this* may be, then, we have our small scattered groups bearing plant or animal names. It is fact once more, that the origin of the names dropped out of remembrance among those who bore them. Now comes the cardinal point of the theory: the insistence on the importance of the name, on the intensity of the savage belief in the intimate and wonder-working connection of names and things. "If each group woke to the consciousness that it bore the *name* of a plant or animal, and did not know how it came to bear that name [and we must remember that this ignorance is fact, vouched for by the occurrence of mythical explanations of the totem names], no more was needed to establish, in the savage mind, the belief in an essential and valuable connection between the human group Emu and the Emu species of birds, and so on. . . Totemism begins in the bearing of the name of an object by a human group." And the connection thus established would be, naturally, the connection of the blood bond. "The animal in myth is thus men's ancestor, or brother, or primal ancestral form. This belief would promote kindness to and regard for the animal."

The essentials of Mr. Lang's theory are, therefore, three: a group-name of unknown origin (which we can ourselves account for, plausibly, as a nickname given from the outside); belief in a transcendental connection between all bearers, human and bestial, of the same name; and belief in the blood superstitions. From these three postulates he is able to derive all the totemic creeds and practices, including exogamy. This last, it will be remembered, was (on Darwin's theory) already in existence as a practical matter from the very earliest times. As to the postulates themselves, the second and third are vouched for by all that we know of "the nature of primitive man."

The author now carries his theory into details, discussing the rise of phratries and totem kins, totemic redistribution, and matrimonial classes. A very curious and interesting point is brought out in ch. viii. On the theory presented, there should be in each phratry a totem kin of the phratry name. Now totem kins of phratriac names occur in America; they had not been observed in Australia. If they cannot be found, the new theory (together with certain older ones) falls to the ground. Mr. Lang is able to show, with some natural elation, that the required totem kins do occur in Australia. "I conjectured that phratry names, now meaningless in the speech of the tribes where they appear, might be really identical in meaning with other names now denoting totem animals in the phratries." The conjecture proved to be correct, and the theory thus receives unforeseen confirmation arising out of an apparently fatal objection. The 'method of inquiry' laid down in ch. ii has brought its well-earned result.

On the whole, it seems to the present reviewer that Mr. Lang has put forward the most satisfactory, because the most completely adequate, theory of the origin of totemism that has so far been suggested.

The argument moves—must move—in the field of hypothesis; but there is good anthropological warrant for each step taken. The pragmatic test is fully satisfied: and what can theorist hope for more?

P. E. WINTER.

The Problems of Philosophy, by HARALD HÖFFDING. Translated by Galen M. Fisher, with a preface by William James. The Macmillan Company, New York, 1905. Price $1.50.

This book is not, as perhaps might be expected from the title, an exposition of the problems of philosophy, if such exposition implies simply a discussion of the philosophical views held by other men. It is rather, as James calls it in the preface, the philosophical testament of the author. And as such it contains his *credo*, along with much acute comment upon contemporary philosophical thinking. But while this intimate character of the book gives interest to it, it is not on that account easy. On the contrary, it requires considerable philosophical training to follow the discourse. Consequently, the book is not likely to be useful to young philosophical readers; it is in no sense an introduction to philosophy.

After a short introduction, the four fundamental problems of philosophy (the problem of consciousness, the problem of knowledge, the problem of being, and the problem of values) are, in turn, considered. The philosophical attitude of the author is designated critical monism. This position is described as striving to "maintain the thought of unity without dogmatizing." It seems to arise from the conviction that the quest for unity and connectedness in experience is forever opposed by discontinuity, and that all accounts of reality must necessarily result in an irrational remainder. The common problem of the book is the relation of continuity and discontinuity; it crops out in each of the four chapters. For instance, in the problem of consciousness, the discontinuous is met with in different mental states and in different individual minds. This discontinuity cannot, however, be transcended (as some writers propose) by a reduction of psychology to physiology. Höffding's own attitude in this instance is that, although the discontinuity is apparent, it nevertheless may not be real, since we can never be sure that analysis has gone to the bottom of the matter. And, furthermore, he seems to favor the notion of a potential psychical energy. In the case of the problem of knowledge, there is again a discrepancy between the principles of knowledge and the being which they strive to render. In this instance, there is an irrational remainder in three forms: in the relation of quality and quantity, in the relation of time to the causal concept, and in the relation between subject and object. Likewise in the case of the cosmological and of the ethical problem, continuity seems an impossible achievement. The problems can never be solved; but in the attempt new thought arises.

H. C. STEVENS.

La femme criminelle, par C. GRANIER. Bibliothèque biologique et sociologique de la femme, No. 12, 1906. pp. ix., 468. Price Fr. 4.

This little work on the female criminal is, within its limits, sanely conceived and temperately written. The limits are of two kinds: the size set for the volumes of the Library, and the assignment of volumes on psychology and prostitution to other authors. It is doubtful whether a reviewer has the right to complain of the plan and scope of the work he is reviewing, and libraries of small, uniform volumes are at present in fashion, especially in France. But it must be said that no adequate idea of the female criminal can be obtained without a study of female individual psychology and a study of prostitution, with its conditions and consequences; and, in the writer's judgment, the reading of three or four small books as against one large one promises no saving of time, while the sectioning of the material may very well lead to artificial boundary lines. An intelligent study of the volume before us is further hindered by the fact that the text is divided up into short, unnamed paragraphs, to which the headings of the analytical table of contents do not correspond; so that it is difficult to decide when one has reached the end of a certain subject, or whether an apparently novel paragraph is merely an excursus within an extended argument. Some of the illustrations are excellent: others are badly reproduced, and of some it may be questioned whether they were not inserted as an after-thought, in order to comply with the Library's demand for an illustrated work.

The first part of the book deals with general criminology: with the statistical differences between male and female criminality, with the influences which favor criminality among women, with the characteristic signs of the female delinquent, and with the question of a criminal type. The second part treats of special criminology, under the headings of maternal, sexual, acquisitive and collective criminality. The third discusses the status of the female criminal before the law, and the attitude of modern society to the question of the punishment of female offenders. The author concludes that "les attributs sexuels irréductibles présentent une différence assez importante pour justifier, en dehors de tout autre argument, l'inégalité de traitement devant la justice pénale."

It may be repeated that the temper of the book is sane and moderate; M. Granier writes from full knowledge of his subject and without bias. He might, perhaps, have avoided to some degree the jerkiness of style referred to above; he can hardly be held responsible for the division of material among the volumes of the Library.

M. W. WISEMAN.

Feeling Psychologically Treated, and Prolegomena to Psychology, by D. J. SNIDER. Sigma Publishing Co., St. Louis, 1905. pp. cxxxii, 402. Price $1.50.

It appears, from the advertisements inserted in this volume, that Dr. Snider is the author of twenty-six books, has a twenty-seventh in preparation, and two others in view. Moreover, the list terminates with an 'over,' which induces the reader to turn the leaf: whereupon he comes upon a blank page, presumably to be filled in the not too distant future.

The reviewer must confess that he was ignorant of all this literary activity until the present work came to his notice. And, if he is to judge from it, he cannot recommend the author as a safe guide in psychological matters. Dr. Snider's position would seem to be that of a panpsychism, strongly tinctured by mysticism; and his method is that of minute logical articulation, such as one finds in the *Erfahrungsseelenlehre* of the eighteenth century. There is evidence that he has read such authorities as Wundt and James, and there are shrewd and suggestive *aperçus* scattered through the text. But the reader who hopes to gain from the book an acquaintance with the affective problems that are now in the forefront of psychological discussion will, most assuredly, come away disappointed.

M. W. WISEMAN.

Leitfaden der physiologischen Psychologie in 15 Vorlesungen, von TH. ZIEHEN. G. Fischer, Jena, 1906. pp. viii., 280. Price, in paper, Mk. 5; bound, Mk. 6.

I have, unfortunately, been able to compare this new edition of Professor Ziehen's Physiological Psychology, not with the sixth edition, but only with the fifth edition of 1900. The work has grown from 267 to 280 pp.—itself an indication that the changes consist mainly of minor additions to text and notes. I have, indeed, found but one omission, and that of very slight range or importance. The principal additions will be found on pp. 50, chemistry of taste; 51-54, psychology of olfactory sensation; 56, temperature sensations; 75, theories of audition; 76, musical scales; 81 f., psychology of tone; 100, contrast and irradiation; 105, sun and moon illusions; 109, perception of form; 112 ff., adaptation and after-images; 127, melodic feelings; 136, sensation and idea; 139, neurology; 151, teleological value of imagination; 154, 158, memory images; 161, secondary feelings; 197, emotive incoherence; 211 f., attention; 217, method of hits and misses; 238, 243 f., reactions; 250 f., expressive movements; 253, infant psychology; 254, laughter and singing. There is also a new fig. (section through the human cortex) on p. 24. The systematic outlook is wholly unchanged. E. B. T.

Studies in Moro History, Law and Religion, by N. M. SALEEBY. Department of the Interior: Ethnological Survey Publications. Manila, 1905, Vol. 4, Pt. 1, pp. 107.

This publication consists of three chapters, dealing respectively with the history of Magindanao, or Mindanao, legendary and recent, with the laws of the Moros, and with the religious orations of the Sulus. Ch. i contains an interesting sketch of the mythology of Mindanao, reminding the reader forcibly of certain tales in the Arabian Nights; translates eight genealogical manuscripts; and ends with a brief history of the island since the advent of Islam. Ch. ii gives an historical account, with translation, of three legal codes: the Mindanao, the principal Sulu, and the new Sulu. It is illustrated by ten half-tone plates of MS. pages. Ch. iii translates the Sulu oration for the feast of Ramadan, and the Sulu Friday oration: it is illustrated by six plates. So far as the layman can judge, the work has been thoroughly and carefully done, and the volume should prove valuable to students of ethnology. H. E. HOTCHKISS.

The Nabaloi Dialect, by O. SCHEERER. *The Bataks of Palawan*, by E. Y. MILLER. Department of the Interior: Ethnological Survey Publications. Manila, 1905, Vol. 2, Parts 2 and 3, pp. 85-199.

The first part of this publication contains a grammar and vocabulary of the dialect of the Ibaloi Igorot, inhabiting a portion of the district of Benguet in northern Luzon: there is added an account of a Spanish expedition to Benguet in the year 1829. The paper gives some interesting notes, with musical transcription, of Ibaloi songs, and among the 22 half-tone plates (from photographs by D. C. Worcester) are representations of musical instruments. The second part describes the primitive people known as Bataks, found in the mountains of the interior of Palawan, about 40 miles north of Puerto Princesa. The paper is illustrated by 6 plates from photographs by the author.
H. E. HOTCHKISS.

Laboratoire de physiologie de l'université de Turin: Travaux des années 1904-1905 publiés sous la direction de A. MOSSO. Extrait des Archives italiennes de biologie, 1905. Vols. 41-43. pp. vii., 337.

This volume contains twenty-six reprinted physiological papers from the Turin laboratory, eighteen of them bearing the name of the director, alone or in collaboration with MM. G. Marro and G. Galeotti. The remaining articles are by MM. C. Foà, G. Galeotti, A. Aggazzotti, A. Herlitzka and M. Ponzo. The greater part of the volume is devoted to the phenomena of respiration and circulation at high altitudes.

there is but one paper dealing with the organs of sense, that of M. Ponzo 'Sur la présence de bourgeons gustatifs dans quelques parties de l'arrière-bouche et dans la partie nasale du pharynx du fœtus humain.'

P. E. WINTER.

Germs of Mind in Plants, by R. H. FRANCÉ. Translated by A. M. Simons. Charles S. Kerr and Co., Chicago, 1905. pp. 151.

The author first points us to the naïve student of nature and the living, perceiving plants of fable and tradition. He gives as proof of the fact that we have become divorced from nature a description of the lifeless systematic Linnean botany. Though there has been for some time a reaction against this, few botanists yet endow plants with sensation. Plants make all the movements their life demands as is shown by the insect eating sun-dew and the hundreds of species of carnivorous plants. Nyctitropism, hygrotropism, geotropism and heliotropism in plants all show this movement. Its *tempo* is much slower than in animals and it often takes patience to observe it. Plants sense odors, flavors, light, vibration, etc., and perhaps many qualities to which we are insensible. Haberlandt has made out several sense organs. Plants give us the best example of reaction to gravity. The starch grains are compared with the statocysts of crabs. The reactions of plants when injured points to the existence of temporary nerve-like elements and Němec has found such in the root of the onion and others. Transmission is also cared for by the protoplasmic tubes.

Sensations must be utilized; is there perception and a soul? The poetry of flowers gives a better conception of the real essence of nature than the exact *veri botanici*. The author's notion of sensation is seen in the statement "All my involuntary movements are released by sensations." Plants become accustomed to different kinds of stimuli. The author is not quite prepared to say that plants feel pain and have a soul, but neither is he satisfied with the position taken by neuropsychology. It does not explain by assigning all to irritability of living substance. There is purposefulness which is not mere teleology. In plants we get reactions which are other than mechanical. The final statement is that plant life is like that of animals and our own. Their sense life is primitive but in it we see a beginning of animal and human mind. Each of these generalizations is illustrated with many concrete examples and drawings. It is the interpretation given to these which one feels inclined to call in question. Various references to the philosophic and poetic make the book, at any rate, very entertaining.

JAMES P. PORTER.

The Changing Order, by OSCAR LOVELL TRIGGS. Chas. H. Kerr & Co., Chicago, 1906.

"The Changing Order" is the change from aristocracy to democracy. Democracy is not a matter of politics only; but is a new spirit of life which signifies the "uprise of the people, the masses." The author starts out to trace the effects of democracy on art, education, industry, and religion; but he does not accomplish this purpose, for he does not distinguish the products of democracy itself from the products of individuals in an aristocratic society who may yet have popular sympathies. Tolstoi, for example, is not a product of democracy, but a reaction from aristocracy. The book is suggestive, however, in tracing various expressions of democratic sentiment. The volume consists of somewhat disconnected essays, several of which have been printed before.

F. A. BUSHEE.

BOOK NOTES.

[Notice of books and articles in this section does not preclude fuller reviews later.]

Experimental Psychology. A Manual of Laboratory Practice. By EDWARD BRADFORD TITCHENER. Vol. 2, Quantitative Experiments. Part 2, Instructor's Manual. The Macmillan Co., New York, 1905. pp. 453.

At last, we have the long-awaited second volume of the author's quantitative experiments. The motto is from Delbœuf to the effect that any phenomena, physical or moral, not translated in numbers, always leaves on the mind the effect of mysticism. The introduction describes the rise and progress of quantitative psychology from Weber. The first chapter, on preliminary experiments, is devoted to tones, pressure and Weber's law. The second, on metric methods, discusses the law of error and the method of limits historically and critically. Other experiments are classified as falling under the limens of continuous change; then Fechner's method of average errors; then that of equivalents; then that of equal sense distances; the method of constant R and of R differences, of right and wrong cases, most of which are treated historically as well as experimentally. The reaction experiments have a chapter by themselves. This topic opens with a discussion of electrical units and measurements and a description of the technique of simple reactions. The three types of the latter are then discussed and this is followed by an experiment involving compound reactions, discrimination, cognition, choice and association. Special final chapters are given to the estimation of time and to some typical experiments showing the range of quantitative psychology. Fuller notice will follow.

A Plethysmographic Study of the Vascular Conditions during Hypnotic Sleep. Dissertation submitted to the Board of University Studies of the Johns Hopkins University for the Degree of Doctor of Philosophy, 1900, by ELISHA CHISHOLM WALDEN. pp. 124-161.

On suggesting hypnotism the arm is slightly constricted and the plethysmographic trace mounts, but falls when the act of suggestion ends. Then for some hours it steadily rises as it does until the end of the hypnotic sleep. On waking there is sudden rise in the curve. The pulse rate is slower during hypnotic sleep than before or after; so is the respiration. The rectal temperature falls slightly. The surface temperature of the arms is higher. "It is difficult to draw any general conclusions as to the bearing of these facts as to the theories of the cause of hypnotic sleep."

Die Lichtsinnesorgane der Laubblätter von DR. G. HABERLANDT. Wilhelm Engelmann, Leipzig, 1905. pp. 142.

It appears that we have not yet sufficiently defined the meaning of the word eye. If it means a local organ for the sense of light, or the perception of light and dark, the leaves of plants have eyes, although, of course, they do not have them if the term is confined to perceptions of form. Both the cuts and the experiments of the author certainly show remarkable things in panphotometric leaves. The author has made many new experiments on the epidermis of leaves and has many cuts, which certainly suggest that there may be some perception of light in the cryptogams, and heliotropism, and afford many interesting analogies.

Geschmack und Geruch. Physiologische Untersuchungen über den Geschmackssinn, von WILHELM STENBERG. Julius Springer, Berlin, 1906. pp. 149.

The writer has here summed up in a concise and admirable way the results of the various experiments upon taste and smell, and added to these some new results and an important new and more precise method. According to Toulouse and Vaschide, Influence de l'age et du sexe sur l'ordorat. Soc. de Biol. 10. 6. 99., sensitiveness to smell develops up to the sixth year and then declines with years. The sense of smell is earlier and more fully developed in men than in women and there appear to be hereditary differences.

The Color Sensitivity of the Peripheral Retina, by JOHN WALLACE BAIRD. Published by the Carnegie Institution of Washington, 1905. pp. 80.

The writer concludes that in moving across the retina the images of colored objects pass through regular tones, the position and extent of the color zones everywhere depending upon the momentary degree of retinal adaptation, brightness, etc. Of all possible colors only four undergo no change in indirect vision—a purple-red, a yellow, a blue-green, and a blue. A color stimulus near the periphery of the retina rapidly fades out. The results are in accordance with the Hering and Franklin theories of vision and cannot be reconciled with any other.

Der moderne Spuk- und Geisterglaube. Eine Kritik und Erklärung der spiritischen Phänomene, von RICHARD HENNIG. Ernst Schultze, Hamburg, 1906. pp. 367.

The author publishes here the second part of a work entitled Wunder und Wissenschaft, with a preface by Professor Max Dessoir. First of all, he candidly presents the sources of error in proving spiritual agencies among which he finds to be the tendency to personify unknown impressions and to imagine perceptions and memories. He recognizes, also, frequently conscious fraud. Passing next to the secrets of trance he seeks to explain what has been called diabolical possession, inspiration, stigmatism, belief in the werwolf and possession by mediums, speaking in a foreign language, Helene Smith and her four chief spirits. In the third part he discusses automatic table movements, psychic activities, raps, haunts, materialization, spirit photography, the astral body, doubles, fourth dimension, scientific authority, and finally the relations of spiritism to psychology and pathology. We must certainly assign to this work a higher and more scientific and generally satisfactory position than we could ascribe to the first part. It is a work which every one at all interested in this subject should certainly know.

The Socialization of Humanity. An Analysis and Synthesis of the Phenomena of Nature, Life, Mind and Society through the Law of Repetition. A System of Monistic Philosophy. By CHARLES KENDALL FRANKLIN. Chas. H. Kerr & Co., Chicago, 1904. pp. 481.

The author first analyzes nature, life, mind and society to get a realistic conception of things. He finds very basal, underlying all of these, the law of repetition. He then discusses the origin of life, the physics of the senses and the intellect, the chemistry of the senses, emotions and will, animal mechanics, realism and idealism, naturalism versus super-naturalism, the expenditure of energy controlled by the mind as a fourth law of motion, control by moral sense a fifth law of motion, and controlled by the social sense a sixth law. He then discusses the supreme law of ethics, religion, the social organism, its dynamics, the hypothesis of God and immortality, the theological social sense, the aspects of scientific morality; finally synthesis of nature, life, mind and society, what the socialization of humanity will accomplish. The final chapters are devoted to forestalling criticism and to applications and conclusions.

Twenty-Third Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution, 1901-1902. J. W. POWELL, Director. Government Printing Office, Washington, 1904. pp. 634.

Mrs. M. C. Stevenson, widow of the well known American anthropologist, here summarizes the result of twenty-five years of investigations of archæologists and ethnologists, which have centered on Arizona and New Mexico. These regions were once densely populated, then desolated, and since held in precarious tenure by the remnants of a dwindling race. The older ruins are in the valleys and here the people lived in prosperity until, by their powerful foes, they were forced to take refuge in caves and cliffs. Many of the stone structures here are centuries old. The author takes up the mythology, worship, rituals, calendar, major and minor festivals, history, arts, customs, games, physical traits, medical practice, witchcraft, esoteric fraternities, and gives us 139 large cuts, many of them colored and full-page, and 34 figures. Her work is largely a labor of love, and of all the precious 22 volumes of the Bureau of American Ethnology, this will always remain one of the most general interest and value.

The Native Tribes of South-East Australia, by A. W. HOWITT. Macmillan & Co., Ltd., London, 1904. pp. 819.

The writer first discusses the origin of the aborigines of Tasmania and Australia, believing it to be the same. Successive chapters then treat of tribal and social organization, relationship terms, marriage rules, tribal government, medicine men, magic beliefs, burial practices, initiation ceremonies of the eastern and western type, messengers and message sticks, barter and trade centres, gesture language. The work is apparently very scholarly, showing wide knowledge of the literature upon the subject, and, what is, of course, far better, bearing everywhere the marks of careful personal investigation.

The Journal of Abnormal Psychology. Edited by MORTON PRINCE, M. D. and others. Bi-monthly, $3.00 per year.

The editors of the *American Journal of Psychology* extend a hearty welcome to their colleagues of the new *Journal of Abnormal Psychology*, the first number of which has just appeared with the imprint of the Old Corner Bookstore, Boston. The field which the new *Journal* represents is perhaps just now the most promising, both theoretically and practically, in the whole range of psychological science, and the character of the editors (Morton,Prince Münsterberg, Putnam, Hoch, Sidis, Dana, and Adolf Meyer) guarantees the quality of the new venture.

The first number contains articles on Impulsions by Pierre Janet, Hypnosis by V. Bechterew, Hysteria by James J. Putnam, and Sudden Conversions by Morton Prince—54 pages in all. A department of current literature will be added in subsequent numbers, to which many able hands will contribute.

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GESELL: Accuracy in Handwriting as Related to School Intelligence and Sex.

FOSTER AND GAMBLE: The Effect of Music on Thoracic Breathing.

PSYCHOLOGICAL LITERATURE.

BOOK NOTES.

American Journal of Psychology.

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THE PSYCHOLOGY OF ORGANIC MOVEMENTS.[1]

By I. MADISON BENTLEY.

One of the strongest tendencies in current psychology is the tendency to emphasize motor activities of the organism. This tendency springs from a threefold root. It rises partly from the modern doctrine of the intimacy of psychophysical relationships, partly from the influence of general biology, and partly from direct analysis of mind itself.

Precisely what the relation between mind and movement is, and precisely what its psychological significance is conceived to be, depend, in large measure, upon the individual's general attitude toward scientific enquiry. Organic movement is apt to wear one aspect when consciousness is interrogated for consciousness' sake, and quite a different aspect when mind is considered as a means to some end;—its aspect and significance depending again upon whether the end be organic welfare, or organic development, or social progress, or the acquisition of knowledge, or conduct, or philosophical construction.

To assume any single one of these attitudes at the outset would implicate us in a partisan treatment of the problem at hand. And, in order to make my discussion as comprehensive as possible, I shall separate the specific psychological problems involving organic movement, which are common to all systems, from the theories—especially recent theories—of consciousness, which are pre-eminently 'motor' in terminology and tendency. The only initial assumption that I need to make in proposing this twofold treatment of what has been indefinitely called 'the motor problem' is that all psychologists, of whatever creed or of whatever school, are, from some point of view, primarily interested in the scientific investigation of consciousness, and this assumption, we may fairly say, is implied in the very term 'psychologist.'

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[1] Read, with omissions, before the Cambridge meeting of The American Psychological Association, December, 1905.

First then, as to facts. The specific psychological problems which have received in their solution overt reference to organic movements may be divided into six principal groups.

I. The general analysis and description of action. Action fills a large chapter in the history of psychology. While early interest centered around its relation to the volitional activity of the individual, *i. e.*, while action was, in the older psychologies, principally identified with will and its expression, the opening of the modern era replaced this general interest by a more empirical study of action for its own sake. Of the two tendencies in this era—the tendency toward analysis and the tendency toward measurement of psychophysical process and function—only one, the latter, made much, at the beginning, of the movement side of action. Analysis of what are usually though ambiguously called the organs of sense, revealed an amazing number of conscious 'qualities,' but analysis of the 'organs of movement' revealed a paucity of conscious qualities; while, on the other hand, nothing offered so good a leverage for the measurement of capacity as the muscles and tendons,—witness the fertility of method for lifted weights and eye movements. And the poverty of movement as regards consciousness was at the same time more than offset by the doctrine of the *Innervationsgefühl*—a persistent echo of the doctrine of an elementary faculty of will.

However, the two methods have not remained separated, dividing equally the spoils of consciousness, but both analysis and measurement now claim a common territory,[1] the whole field of psychology, thus disposing of the fiction of a twofold mind, 'receiving' and 'reacting,' or 'sensory' and 'motor.'

But to return to the specific problems of action. The classical form of action, within the laboratory, is the 'reaction experiment.' A vagrant charge of astronomy and physiology, the reaction experiment became, under psychological direction, a means for measuring the duration of certain mental functions; cognition, recognition, association, etc. Later in its history came the period of interpretation, which brought the distinction between sensory and motor reactions and the famous discussion of reaction-types. Still later came the indiscriminate use of the reaction as a differential test—mental, physiological, anthropological and abnormal,—a use, by the way, to which this association has, from year to year, cheerfully sacrificed its members.

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[1] *Cf.* E. B. Titchener: *Experimental Psychology*, II, Pt. i, 1905, xxxviii, Pt. ii, 1905, 405.

The latest stage of the reaction experiment is the stage in which the experiment is used, both in research and in drill, as a means to the analytic study of the action-consciousness;—for the introspective description and classification of the various forms of action, impulsive, instinctive, automatic, discriminative, and the like.[1]

II. The study of special actions. Within this group fall a great variety of actions which involve some specialized movement apparatus. The most important are, perhaps, speech (vocal coördination, physical and psychophysical phonetics, aphasia, etc.), writing (analysis of writing movements, graphology, diagnostic use of writing in mental disorders), arm movement (conditions of movement, rate, precision, extent), and eye-movement (rate, function in reading, inhibitory effects, laws and relations to space-perception). This rough and incomplete catalogue of special actions suggests the wide range of interest in these special movement-complexes and their multitudinous relations to consciousness.

III. Expressive movements. Again, a rough classification will be sufficient for our purposes. We may divide these movements into *gross movements* (gesture, offensive, defensive, and acquisitive movements, bodily attitude and play of feature), and *minute movements* (changes in the vaso-motor and respiratory apparatus). The range of conscious processes which these movements have been supposed to indicate is as wide as consciousness itself. It is to be noted, moreover, that the employment of expressive movements, in one form or another, is essential to the study of the consciousness of the animal and of the child.

IV. Movement as an essential factor in certain typical mental complexes. Here movement is not regarded as symptomatic of, but as contributory to, consciousness. Emotions and sentiments, rhythm, mental imagery, space-perception, and the perception of self, are all instances that fall under the rubric.

V. Movement regarded as work. This class of problems strongly suggests the influence on psychology of mental pathology. The psychology of work owes most to Kraepelin who, together with his students, has examined the factors, influences and results of work and has devised methods for its registration. French and Italian psychologists (notably Féré and Mosso) have contributed much both to the methods of ergography and dynamometry, and to the analysis of the factors, physical and mental, which influence the individual's working capacity.

VI. Movement regarded as play. I have ventured to place play in a class by itself, although its relations both to expressive movements and to work are obvious. Inasmuch, however, as play involves a special mental attitude in which movement is more than symptomatic and yet less than accomplishment for the sake of accomplishment, it deserves, I think, a special place. Interest in play has centered in classification of plays, in theory, and in pedagogical applications.

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[1] *Ibid.*, Pt. ii, 356.

So much on the *factual* side of organic movement in psychology. Before I pass to the discussion of 'motor' theories, I should like, however, to point out that all these facts just surveyed are common property of all psychologies and that they are, moreover, to be taken into account by any doctrine which professes to give, in motor terms, a general interpretation of consciousness.

It must, at the same time, be borne in mind that the facts do not necessarily demand a general 'motor' interpretation. It is, on the contrary, quite possible to distribute these problems up and down the psychological system, giving each its own particular setting. This mode of distribution is, for example, followed by Wundt in what stands to-day as the most seasoned and the most closely articulated of current systems of psychology. The system offers no single 'motor' problem. Movement is, however, not neglected. It appears again and again; now as a sign of consciousness, now as an essential factor in the perception of space, now as an element in the various forms of action, or as symptomatic of conscious states and processes, or as a condition of fatigue, or as a source of æsthetic and emotive components, or as concerned in language and thought.

But, in contrast to this piecemeal incorporation, we find in various quarters attempts made to consolidate motor problems into one big general problem whose solution is to settle, once and for all, the relation between consciousness and movement. To these general 'motor' theories we now turn. We find that they fall into two classes: (1) theories that refer the character of the total consciousness to the interplay of motor mechanisms and (2) theories that regard the total consciousness as a primary factor in the motor adjustments of the organism. One type of theory emphasizes the motor *conditions* of consciousness, the other its motor *consequences* or *results*.

First, among theories of motor conditions, permit me to recall to you the theory outlined by Ribot in his brilliant essay on the attention.[1] The theory is not new;—Ribot himself finds it in Descartes—but I refer to it for the sake of perspective. Its author stands midway between the earlier English psychologists,—Bain and Spencer,—who emphasized the organic and the evolutionary functions of mind, and a later group, mostly Americans, who owe more than appears on the surface to Ribot's doctrine that the unity and the organization of consciousness depend solely and directly upon organic movement, and that movements and movement-inhibitions represent primary needs or tendencies[1] which subserve organic adaptation.

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[1] Th. Ribot: *Psychologie de l'attention*, Paris, 1889.

Following Ribot, we come upon theories—still theories of the first class—which may be called *central psychophysical* theories of organic movement. These theories seek to state more precisely the way in which the central motor mechanism conditions consciousness. Typical of this class is the *Aktionstheorie* of Münsterberg.[2] The theory substitutes, you will recall, an antagonistic subcortical mechanism of motor discharge for the cortical mechanisms employed by associationists and apperceptionists. This mechanism imposes a dynamic regulation upon the higher centres and, at the same time, affords a necessary condition for all psychophysical functions. Moreover, it is to be noted that the theory not only repeats the emphasis commonly laid upon the integrity of the arc-like functions of the nervous system, but also insists upon the necessity in all central functions of discharge in a single downward direction, *i. e.*, toward the centres for muscular movement. Without entering into the merits of the theory, it may be observed that just as opposing central theories incline to a neglect of this avenue of discharge, the 'action' theory, by overlooking the *general permeability* of the cortex, inclines, in its extreme position, to a neglect of neural tendencies and aptitudes other than motor. Nevertheless, the theory, by laying stress on the general psychophysical *disposition* of the brain and on corresponding mental values and gradations, instead of on mere conscious qualities and their substrate, calls attention to the fact that the study of consciousness is not completed by an analysis into simple processes or into primary functions, but that consciousness must also be regarded as a *state* with a given form, configuration, and with a given tendency and direction of change. It is 'vividness,' the primary characteristic of the attentive state, that is especially provided for in the theory. To make the point clearer, I may be permitted parenthetically to observe that the investigation of abnormal minds has performed its greatest service for general psychology in the discovery that mental derangement and disorder are, in large measure, derangement and disorder of *general state*, and not of any special group of processes. We have come to recognize dream, hypnotic, trance, fatigue, delirious, exalted, depressed states, and the like, and to regard these as aberrant forms of the states of the normal mind.

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[1] These primary needs or tendencies have recently been made the basis of a biological psychology; see G. Spiller in his *The Mind of Man*, London, 1902.

[2] H. Münsterberg: *Psychology and Life*, 1899, 91; *Grundzüge der Psychologie*, I, 1900, 525 ff.

But while we may recognize the importance for the psychology of states of the 'action' theory, we must not overlook the fact that the theory shares its advantage with the older apperception theory, as formulated by Wundt. This theory has, no less, its mechanism for inhibition, selection and synthesis, and where the motor-discharge theory urges the primacy of motor readiness, hindrance and facilitation, the rival theory points to a mass of cortical dispositions and tendencies which are, we may assume, no less important as temporary conditions of the general temper and trend of consciousness.

When we turn to the second class of motor theories—the theories which regard especially the motor *consequences* of consciousness—we come first upon a *general tendency in psychological interpretation* which really takes the place of definite and well-articulated theory. But this tendency (or, perhaps, better, attitude) is so closely interwoven with our problem that it must be taken into serious consideration. It regards consciousness as a forward-moving, constructive process, an activity, whose end or function is organic coördination and organic adjustment.

There is, now, nothing necessarily distinctive about a psychology that regards mind as an activity. Activity psychologies are, and always have been, plentiful. To this class belong, *e. g.*, the Wolffian psychology of faculties, the Herbartian psychology that possessed, nominally, a single faculty, and that still preserves its activities in the writings of Lipps, one of the later trends of English psychology, represented by Ward and Stout, the Austrian school of Meinong and Alfred Fouillée's system based on mental forces.

But the peculiarity of the coördination type of activity is that—if I may use the expression—it 'biologizes' consciousness. It represents mind as harmonizing, adjusting, squaring, adapting to its environment the organism that is fortunate enough to possess a mind. While it has a mixed lineage, it seems most closely to resemble, among antecedent systems, the Spencerian psychology of half a century ago. Herbert Spencer, you will remember, regarded psychology as "a specialized part of biology," [1] and psychological phenomena—when considered objectively—he identified with "nervo-muscular adjustments by which the higher organisms from moment to moment adapt their actions to environing co-existences and sequences." [2]

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[1] Principles of Psychology, I, N. Y., 1890, 138.

[2] *Ibid.*, 141.

The modern type of 'activity' psychology which I have in mind is frequently spoken of as 'functional' or 'motor;' but since it regards mental functions from a single point of view, namely, adjustment,—whereas all psychologies of activity, whether of faculties or not, are, at the same time, in so far as they presuppose an end or goal of activity, psychologies of function, —and since *all* current psychologies share the responsibility of 'motor' problems, it will be better for my purpose to speak of it as 'reactionism.' The term 'reactionism' is suggested both by the frequent employment in 'functional' literature of the term 'reaction' and by the emphasis laid on organic adjustments and adaptive responses to problematical situations. If, indeed, the use of the term needs further justification, it may be found in the significant fact that early presentations of the point of view before us set out from a discussion of 'reaction' in the more traditional sense. I have in mind Baldwin's emphasis laid on the 'circular' reaction,[1] Angell and Moore's[2] 'functional' treatment of reaction, and Dewey's[3] re-interpretation of Meynert's classical myth of the Child and the Candle,—an interpretation that has become no less classical in certain quarters than the original itself.

My interest in reactionism must, at present, be confined to its use of organic movement. Its teaching on this point needs little elaboration. Dewey, in his article on the reflex-arc, just referred to, contends that neither sensation nor organic movement comes to consciousness as specific 'contents,' but only as 'function,' *i. e.*, as one phase of "coördination." "Sensation, as stimulus," he says, "is always that phase of activity requiring to be defined in order that a coördination may be completed"[4] and, similarly, "motion as response, . . . is whatever will serve to complete the disintegrating coördination."[5] Both are parts of an act of coördination and this act is, on the physiological side, a redistribution of 'tensions' and a search for a new neural equilibrium—an aspect of the doctrine that has been dwelt upon in different connections by Bawden and by Judd.

Now the notion of functional interplay of sensory and motor mechanism is, of course, not new. It is prominent, *e.g.*, in the writings of Bain; and Ribot's essay, mentioned a moment ago, is just an attempt to show that motor and sensory factors are inextricably interwoven in the very heart of consciousness; and, once more, no one could admonish more positively against the psychological misuse of the reflex-arc than Wundt in the importance he has laid for thirty years on movement elements in perception, sensorial elements in action, and on both movement and sensorial factors in the state of attention. Physiologists who psychologize are, it is true, sometimes, perhaps often, guilty of a too abstract consideration of the two sorts of mechanism; but I cannot convince myself that the evil effects on psychologists themselves, of the reflex-arc concept, have been anything like as serious as Dewey assumes. An example is made of Baldwin who had, it was alleged, fallen into error in his *Feeling and Will*, but Dewey overlooks the fact that Baldwin had, the year previously, declared that "the distinction between sensory and motor consciousness is largely logical," that "all consciousness is both."[1] Were it a matter of just reprehension for past sins against the 'functional' faith, it might be pointed out that Dewey himself had, in his *Psychology*, been guilty of the heresy that sensations are actual contents and that their twofold function consists in bringing together 'nature and the soul' and in supplying the 'raw materials of knowledge.'[2]

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[1] J. M. Baldwin: *Mental Development: Methods and Processes*, 1895, 132, 374.

[2] J. R. Angell and A. W. Moore: *Psych. Rev.*, III, 1896, 252.

[3] John Dewey: *Psych Rev.*, III, 357; *cf.* T. L. Bolton: *Psych. Rev.*, IX, 1902, 537.

[4] *Ibid.*, III, 368.

[5] *Ibid.*, III, 369.

When we approach reactionism more closely, and ask whether it invents its own 'motor' principles to explain the dynamic, adaptive effects of consciousness upon the organism or whether it borrows these principles from the general literature, we find that its principles are, for the most part, those that have seen much psychophysical service.

We note, in the first place, frequent appeal to the well-established physiological principle of motor discharge. This principle states that all neural excitations tend ultimately toward a motor outlet. The principle appears in various forms; in Bain's law of motor diffusion,[3] in the ancient law of 'excess discharge,' in G. H. Schneider's law of reduction of all movement tendencies to the primary movements of contraction and expansion,[4] and in the law of dynamogeny. The last law, especially, has been used so much by modern reactionism that it requires a word of explanation. The term 'dynamogeny' seems to have been proposed by the physiologist Brown-Séquard,[5] a man (by the way) whose history is closely linked with the history of the institution by whose generous invitation we are assembled. Later it was extended by Féré[6] to cover the variations produced by sensorial stimulation in muscular capacity. The principle of dynamogeny was introduced into our own literature by James and made a special case under the more general law of diffusion.[1] Baldwin,[2] in his law of dynamogenesis, seems to have confused the general principle of discharge with dynamogeny, for he has taken over the former principle, the principle that "every sensation"—to use his rather ambiguous words—"every sensation or incoming process tends to bring about action or outgoing process," and has christened it the law of dynamogenesis. The law, once renamed, furnishes, in Baldwin's hands, a method for child study—through the selection of simple and fundamental motor reactions—and it also supplies the "foundation stone of the theory of organic development." I shall not take time to follow the derivation of habit and accommodation (which involve, also, Schneider's affective shrinking and expansion, and the law of excess) and the adaptive processes of imitation. The important point to be noted is that organic movement becomes, in Baldwin's genetic treatment, not a mere 'expression' of consciousness, but, as an index of pleasure or pain, a means of producing, maintaining and augmenting vital processes.[3] Genetic dynamics—in so far as it may be identified with reactionism—thus falls naturally under our second type of motor theory, the type that pays chief regard to the motor consequences of conscious function.

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[1] *Mental Development: Methods and Processes*, 462.

[2] *Psychology*, 1887, 33 f, 44 f.

[3] A. Bain: *The Emotions and the Will*, 3d ed., London, 1888, 4; *The Senses and the Intellect*, 3d ed., N. Y., 1888, 258, 271 *note*.

[4] *Vierteljahrsschrift f. wiss. Philos.*, III, 1879, 294.

[5] Art. *Dynamogénie* in *Dict. encyclop. des sciences méd.* (quoted by Ribot, *op. cit.*, 178).

[6] C. Féré: *Sensation et mouvement*, 1887, 30 ff.

Another recent motor theory to challenge our consideration is the 'motor-area' theory of Judd.[4] Although this is a theory of the first type, I have reserved it until now because it is closely related to reactionism. Setting out from certain facts of perception and of action, which he describes as coördinations, Judd, in the spirit of Ribot, James and Baldwin, again uses the law of motor discharge to explain the unity of consciousness. But instead of having recourse to the various peripheral mechanisms which Ribot had invoked, Judd turns to the centre and suggests that the common motor outlet of the Rolandic region is responsible for this unity. The theory minimizes the part that kinæsthetic sensations and images play in consciousness, but exalts the organizing function of a centrally situated 'motor' region. The 'motor process' becomes "the condition of fusion of all the coördinated impulses," and thus it is seen to be "not a factor of consciousness, but rather a condition of the unity of consciousness."[5]

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[1] W. James: *Principles of Psych.*, II, 1890, 379.

[2] *Methods and Processes*, 166; *cf. Feeling and Will*, 1891, 28, 281.

[3] *Op. cit.*, 238 ff, 478.

[4] C. H. Judd: *Yale Psych. Stud.*, N. S., Vol. I, 1905, 199; *cf.* W. McDougall: *Mind*, N. S., XXVI, 1898, 159 ff.

[5] *Op. cit.*, 214.

From the present preliminary statements of the theory, it is not quite evident whether its author intends it to be taken as a general theory of conscious synthesis, *i. e.*, whether Judd would agree with Baldwin that "every two elements whatever, connected in consciousness, are so only because they have motor effects in common," or as a special theory of space-perception. A fuller account of the theory will doubtless make this clear. If it is a general theory, we shall be told why the motor apparatus gives us now the spatial type of consolidation, now the qualitative, now the temporal, now the assimilation of 'verbal directions, bell and stimulus' in the reaction experiment, and now some other type of assimilation. If it is, on the other hand, a theory of space-perception, we may expect to learn whether the motor factor accounts only for the unity of neighboring objects in space (as it seems now to do) or whether it goes deeper and explains the spatial pattern itself (*i. e.*, extension together with the variety of spatial relations) as, *e. g.*, Wundt's theory of extensive fusion does. Furthermore, since the assumed motor factor is obviously related to the general state of consciousness,—shown, *e. g.*, in the essential part that distraction plays in 'geometrical' illusions[1]—we may also look for a statement of its significance for a doctrine of attention. In any event, it is to be noted that the rôle of organic movement is not, in this theory, so fundamental to consciousness as in the theories of Ribot and of Münsterberg; for, in these, movement underlies not simply the *unity* of processes but the existence of consciousness itself.

I should like, if I can before I close, to come to more definite terms with the problem which organic movement presents to a reactionistic psychology. Although, as I said before, every movement within the organism which affects consciousness is of interest to the modern psychologist of whatever system or school, it is not to be denied that functional psychology (if you will pardon the use of so ambiguous a term[2]), in so far as it lays emphasis on organic adjustment, whether for the sake of 'genesis' or of temporary adaptation, has a peculiar interest in the motor apparatus and the motor functions of the organism. The reason for this emphasis on adjustment we cannot stop to inquire; whether it is a courteous response to the biologists' appeal for aid, or a primary biological interest in the psychologist himself, or whether biology, which has for many years generously indulged itself in hypothesis and speculation, appeals to speculative minds in psychology, or whether mental activity viewed from a new angle promises important returns to logic and æsthetics and sociology and education and epistemology, we must leave unanswered. The biologizing psychologist is, for whatever reason, in our midst, and the logical relation between his view of consciousness as coördination and his interest in organic movements remains an important part of our task.

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[1] *Cf.* Th. Lipps' 'motor' theory of optical illusions in *Raumæsthetik und geometrisch-optische Täuschungen*, 1897.

[2] The writer has in mind a dozen or more recent definitions of 'function' or 'functional' couched in terms of psychology, of psychophysics, of logic, of biology, of 'energetics,' and of 'pure experience.'

The secret of this relation lies, I think, in the fact that reactionism is at bottom an implicit doctrine of attention. Let us see what evidence for this view the literature offers. Dewey remarks, in connection with his functional treatment of stimulus and movement, that "it is the motor response *or attention* which constitutes that which finally becomes the stimulus to another act."[1] Again he says, "sensation is that phase of a coördination requiring *attention*, because, by reason of the conflict within the coördination, it is uncertain how to complete it,"[2] and in another place he remarks that the stimulus "furnishes the motivation *to attend* [italics all mine] to what has just taken place; to define it more carefully." Attention, then, is the cardinal activity that 'defines,' that 'searches,' that 'discovers,' that 'constitutes' both 'stimulus' and 'response.' In short, it is the activity that "decides how a beginning coördination should be completed."[3] In just what relation this arch-activity of attention stands to the 'minor acts' called 'stimulus' and 'response,' I confess myself to be unable to understand. Mead declares that 'elements' and 'images' are "reciprocal functions" for the "psychical state" which have "now this expression and now that." "What this expression is," he continues, "depends upon the selective activity of attention or apperception—an activity which is practically co-terminous with the psychical state as such."[4] While this passage can scarcely be said to convey a lucid idea of the functional relation of contents to attention, it does bring strong evidence for the relevancy of my suggestion that the all-powerful and all-important activity of reactionism is simply attention regarded as a function. Angell subscribes to this functional view of attention in his recent *Psychology*. Although Angell admits that attention *may* be regarded, as he says, 'structurally,' *i. e.*, as having focus and margin, he frankly declares himself to be chiefly interested in it as "an instance of mental activity," "as a purposive, forward-looking type of action," though he does not think it necessary "rigidly to dissever these aspects." But the fundamental importance of the *act* of attention appears when Angell announces that he means in his book to "regard all the operations of consciousness as so many expressions of organic adaptations to our environment,"[1] and that in adaptation the "actual work of accommodation" goes on at the 'point of attention' which "represents the very heart of consciousness."[2] Finally, Baldwin maintains that "the problem of adaptation is really the problem of selection,"[3] and that "in attention we have, undoubtedly, the one selective function of consciousness," a function which is "the most habitual of all forms of motor reaction."[4]

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[1] *Op. cit.*, 363. [2] *Ibid.*, 368. [3] *Ibid.*, 369.

[4] G. H. Mead: The Definition of the Psychical, 38 (in *The Dec. Pub. of the Univ. of Chicago*, 1st ser., III, 1903).

If we may take these statements as representative of the reactionistic attitude in psychology, we may fairly conclude that, for this phase of the science, attention is the primary mental activity, and that it is primary because its function is to further, through the agency of organic movements, the "adaptive operations of organic life."

But attention, no less than conscious participation in all organic movement, is common psychological property. No modern system attempts to get on without it. Its relation to the motor activities of the organism forms, therefore, a second natural meeting place for reactionism and the analytic and structural types of psychology. For the latter, attention is essentially a state,—not a structure or a process,—with definitely marked characteristics and with important motor conditions and consequences, which are as accurately laid down as present physiological knowledge will allow. For the former, I may repeat, attention is essentially an activity whose operations are directed toward adaptive movements of the organism.

Three inductions remain to be drawn from this comparison. The first is that attention regarded as *activity* stands just as much in need of definition, description and explanation as does attention regarded as *state;* the second is that unless adequate definition, description and explanation are forthcoming, psychological reactionism stands in danger of basing its account of organic movements upon a faculty only less empty and less vicious than were the Wolffian faculties of a century and a half ago; and the third is that reactionism lies under the necessity of formulating a 'doctrine of functions' which shall set into mutual relation the primary functions of attention and all other functions that consciousness may reveal.

Permit me to say, by way of recapitulation, that there is no single 'motor problem' pressing for psychological solution, that organic movements offer, instead, a variety of problems, and that these problems call for a discrimination of facts and theories. The facts fall, as we have seen, into numerous groups which demand detailed and specific treatment, while the theories emphasize either the motor conditions (first type) or the motor consequences (second type) of consciousness. As regards the first type, we have found a current disposition toward the construction of *central psychophysical* theories, but we must note that the actual central conditions of consciousness are so little known that almost any theory may receive recognition, while no theory brings proof or even commands general assent. The theories of the second type, on the other hand, are encouraged by a tendency (chiefly American), to return to reactionism.

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[1] J. R. Angell: *Psychology*, 1904, 7.
[2] *Ibid.*, 64.
[3] *Op. cit.*, 456.
[4] *Ibid.*, 463.

Finally, I must repeat, current reactionism considers organic movement as an adaptive operation issuing from a mental activity, attention, which is ill-defined, and which stands, at present, in danger of becoming a vague ro empty power either of the organism or of the soul.

THE HABITS, INSTINCTS, AND MENTAL POWERS OF SPIDERS, GENERA, ARGIOPE AND EPEIRA.

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INTRODUCTION.

> Il n'est peut-être pas d'Arthropodes, qui meritent, plus que les arraignées, d'attirer l'attention du naturaliste, par leur forme, leur industrie, leurs maneuvres. Buffon (His. Nat., p. 172). Wagner (67).

The web of the Orb-weaving Spiders has long been one of the classical examples of animal instinct. It is so definite and complex, and yet so frail a structure and must be rebuilt so often under varying conditions that, *à priori*, we should expect it to furnish one of the best of fields for the study of the variation of instinct. If the cell of the Honey-bee varies so that, according to Prof. Wilder (73, pp. 654,655), who quotes Prof. Wyman in this connection, there may be a gain or loss of one cell in ten, then from a knowledge of the differing conditions under which the spider works, together with the frailty of the web as compared with the cell, we have as much, and even more, ground for thinking that even greater variation will be found here.

The Spider is proverbially a solitary animal. There are social spiders but they are not to be found outside the tropics. Is the study of the spider to open up the rich mine of individuality and adaptation which Dr. and Mrs. Peckham's excellent work (47 and 48) has discovered for us in the Solitary Wasp? Is the method of "trial and error" a common one with spiders as Prof. Jennings (28) has shown it to be in his recent epoch-making work for the lower organisms? The importance of such a study for Animal Psychology can be indicated by quoting from Prof. Lloyd Morgan (39, p. 196). He says,—"Nothing has been said in this chapter concerning automatism or control in the higher invertebrates, such as the bee or the ant. Their actions seem to warrant the belief that in them, too, there is—besides a mechanism for automatic co-ordination—a mechanism for control. But at present nothing is known of definite control centres in these organisms supposing such centres to exist. There is here a fruitful field for investigation, if we could only find a satisfactory point of departure." Since the above was written results by Forel (26), Von Buttel-Reepen (74), Wheeler (71), Miss Field (22) and others, have been published, which make it certain that the social insects do have such control. Prof. Forel's work (25, p. 48), goes to show that the corresponding nervous control centres actually exist in ants.

Social insects, however, may profit by the example set by others of their kind. Then, too, the environment at least always contains the other members of the hive or colony. To this extent, then, the environment is much the same for all members of the same colony. But it is different with the solitary wasp or spider. Each individual must meet successfully an environment more or less different from that of every other individual. With the solitary animal there is no loss of individual rights for the good of the whole group, such as both Comstock (10, pp. 633, 634) and Maeterlinck (51) have seen fit to emphasize in the case of the ant and the hive-bee. The differences between the bee and ant queen as recently well pointed out by Dr. Wm. M. Wheeler (72) are quite in point in this connection. With the spider, the solitary animal, *par excellence*, the individual must vary or die. Wagner has well emphasized this instinct to separate in spiders.[1] It is irrevocable that spiders should scatter as soon they are ready to leave the cocoon, and after that cope with their little world individually. The effect of this scattering is far reaching in many of their instinctive activities.

The number of our native species has been given by some as four hundred. Structurally considered they show as great differentiation and as high development as many of the insects with which they were classed by earlier zoölogists. From the standpoint of complexity and nicety of development of instinct they are no less deserving of study.

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[1] Wagner, W: L'araignée aquatique (argyroneta aquatica, Cl.). Son industrie et sa vie. Materiaux de psychologie comparée. Bull. Soc. Moscou, 1900, pp. 61-169. This article is in Russian with a summary in French. I am much indebted to M. de Perott for reading the Russian portion of the article to me.

Certainly there is no animal more repulsive to the average person than the spider. This repulsion gives way to one of attraction the moment we get some glimpse of the real life of the animal. Very few, if any of our spiders, are poisonous, the almost universal belief to the contrary notwithstanding. They are not venomous or rapacious as the popular accounts would lead us to believe. The female spiders in ensnaring, poisoning, and feeding upon insects, and even in killing the male spider, may be described as rapacious only by the human observer's too intense subjectivity. They are getting their food by the only method which their evolution has left for their use. One is well within the truth when he says that of all the animals of like organization and habits the mental life of spiders has received far too little attention in a scientific way. In the following paragraphs there will be found a brief survey of previous studies, or such of them as are of interest chiefly from a psychological point of view. There is no other animal about which more has been written that is inaccessible, at least, to the general reader in English.

Historical Orientation.

For a brief account of the work done prior to 1843 the reader is referred to Menge (37, pp. 2-10). This author's history begins with Aristotle and mentions some twenty-seven other writers, the most important of which are Lister (1678) who, according to McCook, is the father of English Arachnology, Homberg (1707), Leeuwenhoek (1722), Clerc (1757), de Geer (1752-78), Walcknaer (1802-05-06-08-37), whose works give accurate descriptions of genera and species, Kirby and Spence (1823), Herold (1824), Oken (1835), Hahn (1830 and 1831), and Koch (1839). These works are largely systematic and given to a description of structure with many observations of value on the manner in which the web is built, how spiders make use of the thread to bridge across chasms, their food, and differences between the sexes.

The use by spiders of nests or geometrical webs to ensnare prey very early excited the interest of observers in the facts just mentioned. Menge himself has sections on food, the manner of spinning the web by the different genera, the sexual instincts, and with this the nest-building and the care of the young, in which he says the female spider of some species shows a love for the young which even surpasses that found in birds. Here we get an example of the anthropomorphism which has colored the observations of nearly all of the earlier writers on this subject, and is not yet extinct. Lastly, there is a discussion of sense-perception, the spider as a weather prophet, and the benefits and injuries to be derived from spiders. In his section on sense-perception Menge gives a description of the anatomy of the eye but says that this sense organ has the most marked development of any in the spiders. The active spiders can see an object a few feet distant. They draw back when one approaches to within three or four feet of their web, especially if they have been previously disturbed. The Garden Spider (*Epeira diademata*) has weak vision and allows herself to be touched before moving. It is doubtful if Menge eliminated jars in his observations just recorded. As to the hearing of spiders he takes a negative position and explains their reaction to music by means of the effect of sound waves on their webs. Their sense of taste is weak but not absent. Disagreeable odors seem not to affect them at all as they do us. The sense of touch in the feet must be very highly developed. He seems to have taken special pains to determine how far spiders were reliable weather prophets. His conclusion is a negative one.

Following Menge there seems to have been little published for some years. Blackwall (4) devoted the whole of his three volumes, except the introduction, to a systematic description. Stavely (64) has also written on the habits of British spiders.

Wilder (73) describes the web of the Triangle Spider and as stated above finds, not in this species, but in his additional observations on Orb-weavers, evidence of considerable variation. Hentz (27) during the first half of the last century wrote many articles on the Spiders of the United States. He may well be called the pioneer in American Arachnology. His efforts were almost wholly directed to systematic description with occasional reference to things psychological, *e. g.*, that vision in one of the *Atti* is acute but not unerring.

Dahl (11-14) has published many articles on the structure of spiders, chiefly with reference to their sense powers. Two of these deal primarily with the mental life of spiders. He tested their sense of taste and smell by holding in front of them, or by actually touching them with a glass rod dipped in turpentine, oil of cloves, or ammonia. He concludes from their responses that they both taste and smell. *E. patagiata* Cl., particularly, perceives odors, discriminates different ones, and these have different affective values for her which are not what they have for us. It will be well at this point to call the attention of the reader to Dahl's manifest anthropomorphism. He believes that there are auditory hairs on various parts of the body, principally on the anterior part, which are the organs of hearing. From the results of tests with flies, bees and pieces of paper fastened to a small wire, he places the limit of clear vision at 2 cm. At this distance some, *Attus arcuatus* Cl., for example, are able to distinguish a bee from a fly. These spiders are able, however, to detect the presence of moving objects at a distance of 20 cm. The color-sense was tested by feeding insects of different colors as well as flies painted with different colors. Two species gave little or no results while two others showed signs of distinguishing the colors. The tactile sense is well developed particularly on the feet and palpi.

In his second article Dahl discusses, (a) Instinct and Cognition; (b) The Social Impulse; (c) The Æsthetic Impulse. He notes that in web-making the spider puts in certain sub-supports when they are needed; also other variations for which external conditions and the properties of the various bodily structures of the spider do not sufficiently account. Therefore these instinctive acts are more or less conscious. He goes yet further in concluding that when, as he finds by actual experiments, *Attus arcuatus* Cl. avoids a certain insect, because she has experienced a few times before, that this kind of insect was covered with turpentine, she is able to and does infer from analogy. He likewise reaches the same conclusion when this and other species avoid beetles and bees after having been fed on these a few times. He thinks that these spiders could have had no experience with this kind of food prior to his tests with them. In some cases he succeeded in inducing them to take flies immediately after they had refused the bee or beetle. He states that their memory for this harmful food lasts at most only a few hours.

Cambridge (8), Campbell (9) and Rainbow (53) are writers on spider habits and instincts from England and Australia. The last named warned against making the web of classificatory value, particularly those spun in confinement. He believes with Wagner (see below) that there may be "fluctuations and variations" of instincts.

M. Eugene Simon (62) has devoted but few pages to spider anatomy and the remainder of two large volumes to a systematic treatment. His programme calls for a third part on Biology such as McCook (see below) has proposed for American Spiders; also a fourth on their Geographical Distribution. He believes that such is the best programme to follow since there are probably many pitfalls in the way of one who knows little or nothing of the anatomy or natural relations of these animals.

Turner (66) noted variations in the webs of a certain Gallery spider. These he considers due to intelligent adaptation; but such is hardly in agreement with the best writers on the subject.

Kennel (32) has made careful observations on the methods by which spiders bridge across chasms or lay down the foundation lines for their webs. He denies that they throw the thread out with such force as to carry it the required distance or that the wind pulls it out. He states that it is fed out with the hind claws. The air currents depending on their strength and direction determining in part the point of attachment of such a thread.

The work of Dr. and Mrs. Peckham stands perhaps as the most extensive and thoroughly reliable from a psychological point of view. In their work on the Mental Powers of Spiders (41 and 42) they have made carefully controlled tests and observations on the senses of smell, hearing, sight, the color-sense, the so-called instinct of "feigning death," the maternal emotions, and the mistakes of spiders. The sense of smell was tested by holding a glass rod with its end dipped in oil or perfume in front of the spider. Each test was at once checked by holding a clean rod in the same position. Many tests were made with different spiders and the authors worked separately. Their conclusions are that only three species fail to respond to the test. It was evident by their various movements that the scent was perceived by all the others.

The sense of hearing was first tested by making loud noises, shouting, clapping the hands, etc. To these there was very little, if any, response. They then experimented with vibrating tuning-forks as Boys (6, p. 149) had previously done. After many tests made with forks of different vibration rate and checked by bringing the non-vibrating fork into the same position, they conclude with Boys that spiders hear. They get some responses after removing the palpi and parts of the foreleg. Yet they do not agree with Plateau (40, p. 384) that palps are useless organs. According to them the end-organs for hearing are not well localized. However, it is not certain that jars have been eliminated and their failure to elicit any response in active spiders points to the fact that the web takes up the vibrations and the spiders feel these.

However, Pocock (50, p. 63), contrary to McCook and the view just stated, thinks that spiders hear and that the tuning-fork experiments have been tried in a sufficiently variable manner to constitute satisfactory proof. I have made no test with the tuning-fork, but have failed to get responses to loud noises.

Some spiders, notably *Cyclosa conica* and *Epeira strix*, responded to the vibrating fork by suddenly dropping from the web. After many experiments they failed to get this response after each test. With a single *E. strix* with which they worked for more than a month there was an increase in this power of control, thus giving evidence of a short memory. This failure to show the so-called "feigning" instinct is regarded by the authors as rather remarkable, as it is undoubtedly an instinctive reaction of great value to the animal.

These same authors conclude from seeing the males in search of the females and the females for their cocoons that the range of vision for the active spiders may be as great as some eight or ten inches. This is much greater than Forel (24) has put it. Their tests on the color-sense were made by noting, under duly controlled conditions, which compartments surrounded by red, green, blue, or yellow glass the spiders preferred to stay in. They also tested the color-sense in males by painting the females and observing the different reactions of the males, the female without paint having first been placed before the several males; also by separating the female from her eggs and nests and surrounding these with pink, blue, or red paper. The female seemed to become accustomed to finding her nest framed in blue and wanted to return there even if the blue had been changed in place and pink then surrounded the nest and eggs. From all these experiments the authors infer that spiders see colors and the first series indicated a preference for red "much more marked than that found by Sir John Lubbock for ants, and the spiders had not so positive dislike for blue."

Various alarming stimuli were given to spiders by these authors in order that they might observe the so-called "feigning" instinct. This reaction consists simply in dropping from the web or nest and lying with legs flexed as if dead. Their findings from more than two hundred experiments on nineteen different genera may be indicated as follows (41, pp. 416-417): "Out of the species with which we experimented we found one which would endure a moderate amount of pricking with a needle, and a second which did not move when its legs were pinched. Beyond this there was no stoicism under anything that approached bad treatment, although a few species allowed themselves to be handled without showing signs of life. We do not believe that any spider which came under our observation ever fell into a Kataplectic condition. . . . There is no need to call in 'Kataplexy' to explain the origin and development of a habit which can be easily explained by natural selection alone." Robertson's (54) conclusions agree with the above. He holds that for active spiders the stimulus producing the "sham-death" reflex must be sharp and sudden. With the sluggish the reflex posture is more continuous and practically independent of the nature of the stimulus. In the active the reflex may be carried out by the thoracic ganglia alone, or even by the ganglia of the two posterior or anterior segments. In the sluggish it cannot be induced without the head-ganglia.

The "sham-death" in intact spiders is a complete tetanus. With the supra and sub-œsophageal ganglia removed the reaction is carried out but is weaker, in at least one of the active species; it has a longer latent period and it is a rhythmically interrupted tetanus. This reflex is probably a means of escape in emergency, and "conscious volition" can have nothing to do with it unless consciousness can be an attribute of each thoracic ganglia.

The Peckhams also found that those spiders which carry their cocoon about with them will take up a pith-ball or a web-covered shot instead. But when allowed the choice between a pith-ball and their cocoon they take the latter in preference, provided they come into contact with the cocoon. This proves that, since these spiders carry their cocoon underneath their body, they are unable to recognize it by sight alone. Tactual stimuli must be present for an effective perception. They also infer that the muscular sense in these spiders is poorly developed since they persist with much difficulty in carrying about the heavy shot. But the instinct in which this act finds its setting is necessarily so strong and important for the preservation of the species that we should be slow to make such an inference. However, this species was not an Orb-weaver, and need not have so finely developed a muscular sense.

These same authors have published three different articles (43 and 44) on Courtship in spiders of the family *Attidae*. They describe and give drawings of the antics of the males and in the last article discuss the bearings of their results on the Darwinian and Wallacian theories of selection. They maintain that their observations uphold the theory of sexual selection as stated by Darwin.

McCook (35 and 36) in his many articles published by the Philadelphia Academy of Science and his three large volumes "American Spiders and their Spinning Work," treats their natural history with special regard to their industry and habits. Of special interest to us in the present paper is the fact that he counts and measures a few webs of some species and finds evidence of considerable variation. He also brings together the previously known results in their appropriate places. His large works are popularly written, and although he is anthropromorphic in statement, he is somewhat loath to grant intelligence to those spiders he has most studied.

Prof. Emerton in his two excellent books (19-20) and many earlier articles deals with the structure and habits of spiders common to the United States and often notes as well the variation in the number of parts of the web, together with interesting observations on the sexual instincts, etc.

Wagner (67 and 68) is certainly one of the most careful writers on spider habits and intelligence. His mental attitude toward spider activity reminds one strongly of that of Bethe toward ants and bees. They are looked upon pretty much as "reflex-machines." As suggested above he finds variations in instinctive activity. These are great enough to furnish a foothold for natural selection. He gives a few observations which support his view that such variations have their origin in the germ-plasm. He is certainly right in explaining many of the adaptations as instinctive. But in denying all intelligent power to spiders he is perhaps taking an untenable position.

Prof. Montgomery (38) with much painstaking and careful observation has described the mating habits of many of our common spiders. Very few of the orb-weavers were included in his list. He considers that the approach to the female by the male is a very interesting field from a psychological point of view. He holds that the courtship of the male is more remarkable since he must re-charge the palps with sperm. To this he has given name of *sperm induction*. He concludes that the male is guided by tactual and visual stimuli in his search for the female and that the strongest male wins. The female exercises no conscious choice in her selection of the male.

Lécaillon (34) describes the nesting habits of one species which lives inside her nest with the young. This author tests the strength of the maternal instinct by separating the proper mother from her young and noting her attempts to replace a foster mother. He finds the maternal instinct strong and that it lasts for some seven or eight days.

Miss Pritchett (52), in addition to giving a good bibliography and a review of the work already done on the senses of hearing and smell, obtained, by careful tests with a tuning-fork and other methods, a negative result for hearing in the two species tried. She thus confirmed the results obtained by the Peckhams for active spiders. Irritant and non-irritant oils were used in the tests on smell. Both males and females responded to both these even when the palps, hairs on the legs, or the first pair of legs respectively, were removed. Hence the end-organ for the perception of odors must be distributed more or less over the whole body.

METHODS OF STUDY.

The species chiefly dealt with in the present paper are: *Argiope transversa*, *Argiope riparia*, the Shamrock Spider—*Epeira trifolium*, the House Spiders—*Epeira Sclopetaria*, *Epeira strix* and *Epeira patagiata*. The results of some observations on a few other species will also be given. For description of most, if not all of the species herein mentioned, the reader is referred to Professor Emerton's work (20) whose nomenclature I have used throughout.

The first problem selected for solution was the determination by actual count and otherwise of the range of variation between webs made by different members of the same species as well as between successive webs of the same individual. In order to do this most conveniently it was thought best to bring as many spiders as possible into the laboratory. If this was done, they must be given water to drink (since it has long been observed that they naturally take a good deal of water), various insects for food, and suitable places to build their webs. The plant-stalks bearing the nests or to serve as main supports for the webs were usually placed in bottles containing water and these placed on tables or wide window-sills. Other stalks were placed near so that the spider might find at hand as nearly as possible the same conditions as she would have had on the outside. Each spider was numbered and, since many different rooms were used, some very large, the spiders were correctly traced in most cases when they wandered, as they often did, from their assigned places. As there is great variation in size and markings between the individuals of the same species that may be brought in at any one time, they were the more easily followed. During the first season's work almost every spider observed was killed and preserved in alcohol. These were later submitted to Professor Emerton for identification.

For some of the *A. transversa* (this species not having a nest but, while not at work standing in the centre of the web,) boxes and old aquaria frames with removable glass sides and top were used. However, these were soon discarded for the reason that they often led to the building of an abnormal web. the spider bending the upper half almost at right angles with the lower if the top of the case was in the way. Toward the close of my work in late autumn the spiders and the surrounding grass were well sprinkled with water from an atomizer. This is a necessary precaution if these animals are to be kept normal.

Photographs of many of the webs were taken and drawings from some of these will be given. All these drawings are, therefore, very accurate, the lines in the negatives being very carefully followed in each case. It is especially difficult to get good photographs of webs unless one uses Emerton's method of sprinkling them with a solution of shellac in alcohol. This I could not do as it would probably have made the conditions surrounding the spiders altogether too abnormal.

From what has been said it will be seen that the various habits and instinctive activities, indicated at the beginning of this paper, were open to observation and study, both in the field and the laboratory. Many observations on *riparia*, especially on the mating habits, were made during the last season near Hoopeston, Illinois. It should be clearly kept in mind that two brief seasons are too short a time in which to gather data which will support conclusive generalizations. It has, nevertheless, seemed worth while to publish the data gathered together with the tentative conclusions which they suggest.

CHOICE OF THE PLACE FOR A WEB, THE NEST AND MATERIAL FOR THE NEST.

In the pseudo-scientific literature the statement is often found that spiders exercise great intelligence in choosing the place for the web. The different sorts of places chosen by the same individual are many, and it is well to look into the matter more closely before believing (Büchner 7a, 80, p. 315) "that spiders know just where to place their web in order that most insects may be ensnared by it. Practice, experience and reflexion must also guide the spider in the important choice of the locality in which it shall spin its web, in order to catch the largest amount of prey. Before all it likes those places where the rays of the sun and dancing midges may be united with the possibility of a hidden retreat for itself, or where a slight draught blows flying insects into the outspread nest, or where fruit attracts them."

Indeed, if such were the case we might well say, that the spider shows the highest degree of intelligence. But while practice and experience may be worth something, my observations of web building have inclined me to accept Wagner's (67) explanation as nearer the truth. The determining factors are, (1) The place where the particular species concerned habitually lives or hunts, (2) The shape of the foundation of the future nest, and, (3) The peculiarities of the spider's organism. As is evident from the form of statement Wagner speaks primarily of the nests of spiders and not of their webs. We fail to see in the above any room for any psychic factors other than those which may be called instinctive. In placing the web imitation of the old by the young is impossible. None of the young of the species here considered ever see the old, and if they did their range of acute vision is too limited to allow them to profit by the example of the old. Apparently the young from the first choose the place for their web in the same way as the old. To be sure, they must be governed somewhat by their own size for upon this the size of the web in a measure depends. But the latter is also determined by the place chosen. This is in accordance with the third factor mentioned above. Again, although spiders wander some, the large sluggish kind here considered do not go far. For example, a space of low land near a brook not more than thirty feet square had at one time 17 *E. trifolium*, 12 *A. transversa*, 2 *A. riparia*, 2 *E. strix*, 2 *E. corticaria*, besides a large number of more active kinds. The second factor, the shape of the foundation of the future nest applies as well to the web. It is, in fact, the most important of all. Concrete examples will illustrate this as well as the others. *A. transversa*, No. 106, was brought in and placed in the same bunch of grass in which she had spun her webs in the field. This was done by digging up enough earth to hold the grass roots in place. This was placed in a small aquarium frame so that the grass stems might be the better supported. There were no glass sides or top. Her first attempt was to build from the end of the frame to the top of the table. In this case a line from *a* to *b* in Fig. 1 would have been the top horizontal foundation line of her web. Her first web spun in-doors was in the grass as shown in Fig. 1. This lay in a plane at right angles to that which the first would have had. She began this web only after more than an hour of wandering about. Fig. 2 shows her third attempt and second web. This, as may be seen, is in as different a position from the second as this was from the first. Fig 3, shows her third web, much smaller and higher up than the preceding. Here she spun several webs and then the last one from the side of the frame to the top of the table. The degeneration in the successive webs I am certain is due largely to the fact that she found less and less firm supports, and smaller space in which to hang her web. In the first two attempts described above, where I saw her choose the place for her web, her method was clearly one of try and re-try, in the first having almost laid down the third line to the web and yet giving up for that day. *E. trifolium*, No. 48, was seen to persist for some time in an attempt to lay down the fourth foundation line in such a way as would have enabled her to make a web of the typical form and size. Failing in this she seemed to make the most of the possibilities.

The place for and the material out of which to make the nest which *E. trifolium* and the House Spiders use would seem to be chosen in exactly the same manner. The former will bend over one or more heads of grass, Fig. 4; she may, and usually does, use leaves or blossoms of golden-rod, the leaves of birch or of some weed. *E. strix*, *corticaria* and *angulata* usually select leaves which they also bend into a bell-shaped nest. Yet *E. strix* may use the blossom of Yarrow with no silk lining at all, the leaves of ironweed, or pull together into a bunch some heads of fine grass. She even occupies the abandoned nest of *E. trifolium*. Any of them may make use of any corner, or crevice, which will furnish them with the thygmotactic stimuli over the surface of the body which seem to make them feel more at home. This would mean that they

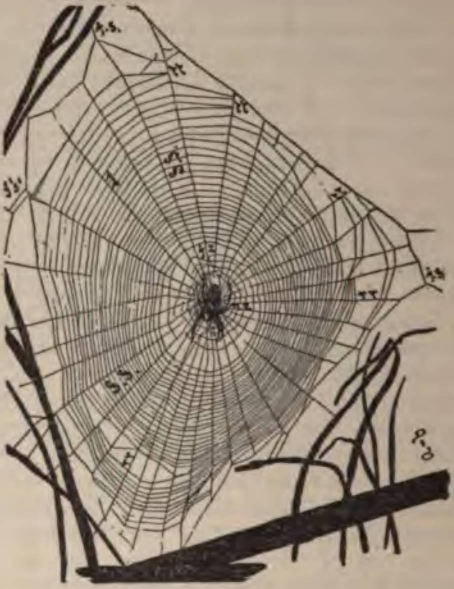

FIG. 1.

Fig. 1 represents the first web spun indoors by *Argiope transversa*, No. 106. It, like all the figures in this article except 6 and 7, is a drawing by Dr. M. T. Thompson from photographs made by the author. *f. s* = foundation zone; *rr* = radii: *ss* = the main outer spiral space; *f.z.* = the free zone; *n. z.* = the inner zone; *tt* = the turnings about; *f.' s.'* = extra sub-supports, and *a—b* represents the top horizontal line of the first web which No. 106 attempted to build.

are positively thygmotactic during the day. When night comes, they move out and stand in the centre of the web. Though *E. trifolium* habitually stays in the nest during the day except when wrapping prey, I have had three under observation which, on finding no convenient place for a nest, stood in the centre of the web *à la Argiope*, detected the presence of prey, and wrapped it in a perfectly normal manner. Though the nest is normally 30°-40° from the vertical and above the centre, Fig. 4, it is not always so. It may be below the centre or to one side. This is yet another indication that its location may be a secondary consideration.

It is evident from the facts just given that the species here considered use for the nest whatever place and material comes most conveniently to hand. Their instinct must allow them a wide range of selection. Intelligent choice of either is apparently out of the question. Wagner (68) states that the water spider (*Argyroneta aquatica*) shows no selection of water plants. They are always those gathered together by the elastic threads which she, like all spiders, always spins as she

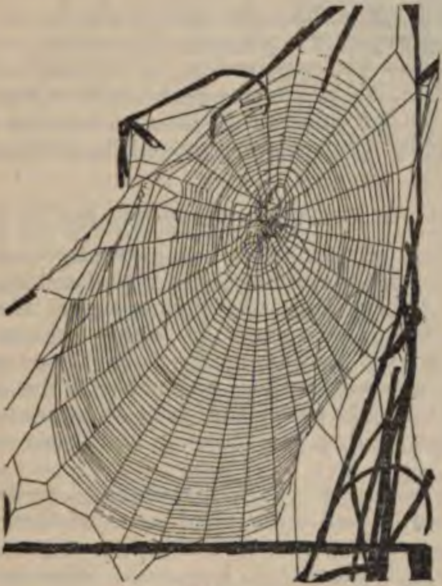

FIG. 2.
The second web made by No. 106 in-doors. Note the difference from the first, Fig. 1.

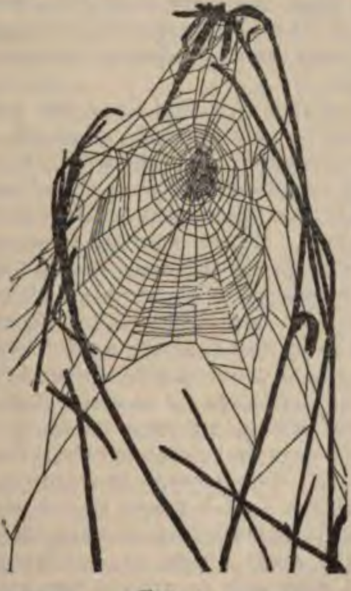

FIG. 3.
The third web made by No. 106 in-doors. The effect of change of place is marked.

wanders round. She, like the spiders above, may build in a plant, in a depression in a piece of rotten wood, or in an empty shell. Such was his explanation of McCook's observations on the Turret Spider which was found to have made use of the cotton, used by the collector to close an opening in the lower part of the spider's turret, for a part of its underground dwelling.

THE MANNER OF BUILDING THE WEB AND ITS VARIATIONS.

The preceding section has led us naturally to wonder if the spider shows a like adaptation in the actual making of the web. As is clearly evident from what has been said of early writers on spider habits, the manner in which spiders build their webs has been fairly well known for a century or more. Therefore, the aim here will be more to emphasize the variations to be found, giving only such other facts as are necessary to furnish a sufficient setting for those in which we are primarily interested.

Before proceeding with the variations to be found in web building, it will be well to fix upon some of the terms used in the description of the web. I shall use, so far as possible, the nomenclature of McCook (35, I, p. 54). That part of the web marked *f. s.*, Fig. 1, is the foundation zone where are placed the foundation lines, usually stronger than the other lines. The radii, *rr*, are the lines radiating from the centre. The spiral space, *ss*, is that of the main outer spiral. The space, *f. z.*, is the free zone. The inner zone is indicated by *n. z.* over which the spider is standing.

The spider's first task is to lay down the foundation lines of the web. She may be able to walk completely round the place in which she has chosen to place her web. If so, she holds the thread away with one hind claw from the objects on which she is walking, pulls it taut, fastens it at the corners, and she has the sides of her web. These may be three or more. If she must bridge over a chasm, she does so by feeding out a loose thread from her spinnerets with her hind claws until it strikes some object. In the semi-darkness in which I watched my spiders I learned to expect that the spider would make a start directly if she began to move the hind feet in this feeding-out manner. Her tactual sense is so acute that she detects the moment the thread strikes, at once draws it taut and travels along it. Some writers have claimed that the spider allowed the wind to pull out this thread, she only starting it; others have thought that the spider threw it out with force enough to carry it the required distance and attach it to some object. But Kennel's (32) observations are in favor of the method first stated, and I have seen *transversa* repeatedly feed out the thread. The distances bridged are too great—being sometimes 30-40 feet,—to be explained by either of the other methods. It will be seen that by this means the spider may travel from place to place through the air as well as make supports for her web. The spider varies from one method to the other supposedly because of the character of the objects on which she builds. In case webs are hung to horizontal threads bridging wide spaces, these threads are doubled and redoubled until they are remarkably strong.

In making the web the main supports are the first to be put in. Kirby and Spence (33 a, p. 232) suggest that some species may put in some of the radii first. A single observation of *E. sclopetaria* would point to the same as being true of this species. Then the spider lets herself down from about the middle of the horizontal support at the top and thus forms the first of the radii. After this the radii are put in from the centre, the spider walking along an old ray and holding the one just being spun out with one foot so that it will not touch and adhere to the one she is walking on. After fastening it to one of the side supports she walks back along it and perhaps doubles it as she returns. The order in which she puts in the rays is not without interest. The general plan is to alternate from above to below the centre and from left to right, though this order is not a rigid one. Some have said that the spider has no appearance of measuring on the supports in order to see where to fasten the radii. My observation is that she does have such an appearance. Spider No. 106 saw fit to cut some of the lower radii which as first put in would have thrown the lower part of her web much out of line, Fig. 1. She did this by cutting them at the outer ends and swinging down on them until she struck the grass below. This made the entire web lie more nearly in the same plane. After the radii were all in No. 106 proceeded to test them. Her manner of doing this was to face in turn each group of radii and pull sharply several times after which she strengthened some of them. Above and below the centre in Fig. 1 may be seen three or four radii which are unusually strong. This, as well as the number of radii, indicate that the spider in building her web balances one side against the other. In the above example the grass supports were probably somewhat less stable than is usually true of those used by spiders in their natural habitat. Yet, judging from the finished product, cases not unlike this happen on the outside as well. No. 106 found that at one time she needed a sub-support, *f.' s.'*, Fig. 1, and stopped to put this in, taking up the interrupted work again at the right place. Her next step was to put in the spiral from the centre out, Fig. 4. This serves her for a guide when she later begins, usually at the lower outer edge, to put in the sticky spirals. The guide lines are cut out as they are reached by the spider when she is putting in the sticky parallel lines. As she places these latter we see that now if ever the spider needs all of her eight claws. One cannot but believe that here there is need of the nicest correlation of tactile and muscular sensations together with the most delicately co-ordinated muscular contractions. It is a revelation to see an *E. angulata* in almost total darkness fastening these outer spirals to the radii at the rate of almost one per second. In fact spiders are said by most writers to spin as perfectly in the dark as in the light. They must, therefore, be guided largely, if not wholly, by tactile and motor cues. There can be no doubt that here the spider measures, one claw always marking the place where the spiral is clamped to the ray by the spinnerets. All my observations would indicate that in putting in these outer spirals the spider goes round in a direction contrary to the hands of a watch having put in the guide lines in the opposite direction.

Before quite reaching the centre the spider ceases to put in outer spirals and to cut out the guide lines which here are very near each other. This space without spirals is the free zone, *f. z.*, Fig. 1. It is left so, according to McCook (35, I, p. 58), in order that the spider may easily pass from one side of the web to the other. The few cases I have seen would indicate that the spider may perform this act very quickly.

After the parallel lines are all in the *Argiope* spread the spinnerets and plaster or darn the centre with wide strands of web. As they have no nest and habitually stand in the centre of the web this gives strength to the web and also a shield to stand behind, Fig. 6. However, this darning of the centre is often omitted. The *Epeirae* do not plaster over the centre but gnaw out the fuzz collected there in drawing taut the radii, etc., Fig. 5. Fig. 4 shows a failure to do this. But before the *Argiope* take their position in the centre, they go directly below and turning round to pass upward, spread the spinnerets, throw the abdomen from side to side, and make the zigzag or "winding stair," Fig. 6. There is also an upper half to this structure. Yet, as is shown in several of the figures, the whole or a part of this is frequently absent, the upper half more often. Male spiders of *A. riparia* have sometimes the faintest suggestion of a zigzag.

The function of this part, according to McCook, is to strengthen the web. May it not be also to aid these silver, black, and yellow spiders as they stand astride of the zigzag to a better mimicry of the plants about them, and thus be neither conspicuous marks for birds nor apparently different from objects which insect prey alights upon? However, it is so incon-

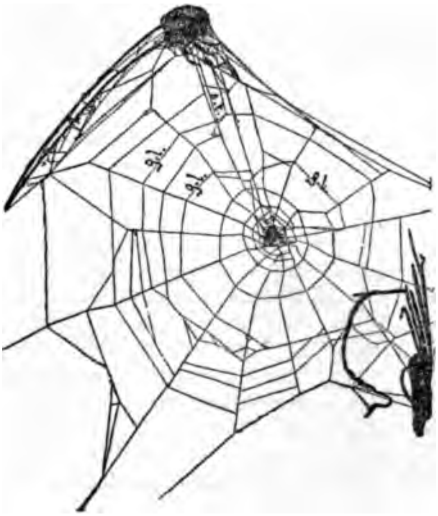

FIG. 4.

Fig. 4 represents a half-finished web of *Epeira trifolium* showing the guide lines, *g. l.*, also the failure to gnaw out the fuzz in the center.

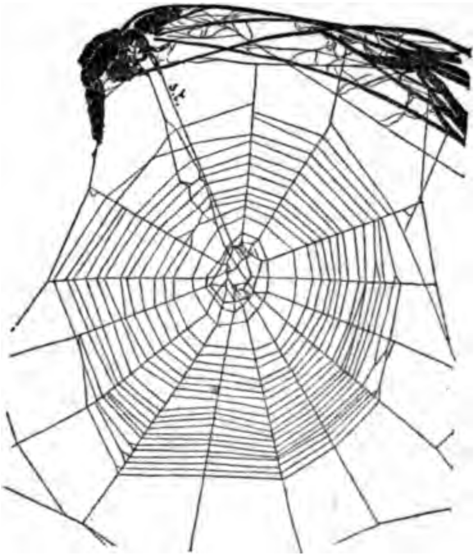

FIG. 5.

This and Fig. 4 show the imperfect nest this spider often makes, as well as her usual position with one fore-claw on the signal thread, *s. t.*

stant that its value, whatever it may be, is at present somewhat questionable.

Many of the webs of *Argiope* have what are called side-screens or guards. One such is shown in Fig. 9. These serve for protection and to cause insects to fall into the web. They would seem to be absent whenever there are no suitable objects near by to which to fasten the threads. They have been absent in whole or part from most of the webs I have seen.

Another adaptation not always required is to tie back the grass in order to make a place for the web. This is done by *E. trifolium* as well as by the *Argiope*.

The webs of all spiders here considered have a normal slant of about 30° from the vertical. This slant is of much service to the spider. The web catches falling objects as well as flying ones. A more important consideration, however, is that when the insect is cut out it swings free from the web, hangs to the spinnerets and hind claw of the spider, and can be more easily carried up to the centre or to the nest. They have an awkward time of it if, as I have seen, they attempt to carry the insect on the upper side of the web. The webs vary within the widest limits as regards slant. That the slant given to the web is of the hit and miss sort with the constant corrective of the general instinctive tendency toward the normal position, is evident from what was said above of No. 106. No. 23 of the same species spun a series of some 10-12 webs in the same branched twig and no two had the same slant.

The repair of webs by spiders has furnished material for much discussion. This seems to follow the same method as the building of the web as a whole. My spiders appear to illustrate again what Wagner has pointed out for nests of his water spiders. He purposely injured the nest to see what the spider would do. Their method of repair was like the making of an entire nest. Where the spider would be expected to use her intelligence she does not. In repairing the web *Argiope* may put in the repair at a different slant, suggesting that she is proceeding by her method of building an entirely new web. Often one sees webs of *Argiope* with two zigzags, Fig. 6. This shows how stereotyped is the connection between different acts of the web-spinning process. The two zigzags indicate that the spider in making the new web felt compelled to complete a new zigzag though she had not cut out the old one. This cutting away of the old web is the point at which Prof. Emerton thinks the spider offers us one of her very best examples of intelligence. When she comes to the last act, the making of the zigzag, which now she need not do, a second one is put in, and, as shown in Fig. 6, it often does not coincide with the first. These two zigzags are often found in *A. riparia* webs in a

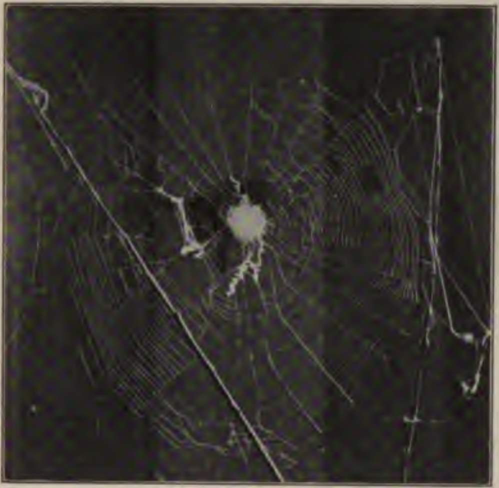

FIG. 6.

A photograph of No. 23, *Argiope transversa*. This shows the quick adaptation of which this spider is capable. (See page 337.) Note the double zigzag and re-darned centre behind which the spider is standing; also the wide strand which the spider spun as she chased the fly over the web; the wrapped fly and the strand with which she anchored it to the centre. She has two feet on this anchor thread.

state of nature. If the animal were exercising intelligent control would she do this? Again, the act just preceding the making of the zigzag, that of "darning" the centre, as I have called it, is gone through with in the same way until the centre is so well "darned" that the spider on the other side is hardly visible, Fig. 6. The fixed sequence of tactual or muscular impressions (or both), which guides the spider in making the web at first, probably impels her to go through all the remaining parts of the process.

The young of spiders are said by some to build imperfectly, but both Blackwall and Wagner deny this, Romanes (55, pp. 216, 217). The webs of the young are small but very perfect. I refer especially to the young of *E. sclopetaria*. I have seen many of these spinning their webs in torn places of the web of a larger spider. They work as rapidly as the old, and the finished web is as perfect. Indeed, according to my own observations and, as I have seen stated elsewhere, their webs may be more perfect than those of the old. This might well be from the fact that the sticky lines of the web are not pressed together by their slight weight, and the web being so small (often no larger than a half-dollar), it is not so dfficult to find a suitable niche for it.

There are thus many adaptations to particular circumstances to be seen in the above examples, which I believe are fairly typical. Many spiders, no doubt, find weak supports for their webs, as must have been the case with No. 106. There can be little doubt that, since spiders of even the same species must build where the surroundings are widely different, they must have this instinct so plastic in certain particulars that the individual can make much use of the "trial and error" method. This, as will be shown later, need not involve conscious adaptation. Yet where the conditions are such that there can be less change, as gravity for example, and certain directions, after the centre of the web has been once located, the actions are more stereotyped and fulfill in part the requirements of "tropisms."

A QUANTITATIVE MEASURE OF THE VARIABILITY OF INSTINCT AS DETERMINED BY COUNTING THE ELEMENTS IN PARTS OF WEBS.

In order to get a more exact measure of the amount of variability in this web-building instinct, counts of five different parts of the Orb-web were made. These were as follows: (1) the number of supports or attachments to objects; (2) the number of radii; (3) the number of outer spirals above the centre; (4) the number of outer spirals below the centre; (5) the number of inner spirals inside the free zone. As many successive webs as possible were obtained from each individual spider. Series for *A. transversa* and *E. trifolium* for both in- and out-of-doors were obtained. With *E. sclopetaria* separate counts were not necessary for the reason that she is equally at home in either place. A few counts were made for different individuals of *E. strix*, *patagiata*, *corticaria* and *A. riparia*. As the latter is very closely allied to *A. transversa*, and my general observations have been perhaps more extended with her than any other, I am certain that what is shown to be true of the *transversa* webs will be found to be true of *riparia* when actual counts are made. "Outside" and "inside" indicate that the webs were spun either in-doors or out-of-doors. Av. stands for the average, M. V. the mean variation, and Ex. V., the extreme variation.

Epeira trifolium.

In field.			Supports.	Radii.	Outer Spirals.		Inner spirals.
No. of Spiders.	No. of Webs.				above	below.	
		Av.	12.15	23.07	14.46	27.02	8.38
20	90	M.V.	1.94	2.28	2.92	3.62	1.11
		Ex.V.	7	13	3	7	4
			19	31	23	37	11

Inside. No. of Spiders.	No. of Webs.		Supports,	Radii.	Outer Spirals. above	below.	Inner spirals.
16	42	Av.	10.28	18.9	9.06	17.88	5.27
		M.V.	2.19	2.79	3.16	4.29	1.09
		Ex.V.	7	12	3	5	3
			24	25	21	32	9

Individual E. trifolium.

	Supports	Radii	Outer Spirals above	Outer Spirals below	Inner spirals
Inside.	7	20	—	20	5
	7	22	—	20	7
Spider No. 3,	9	23	15	20	8
	13	22	12	19	5
	11	22	15	24	5
Spider No. 12,	10	21	13	24	7
	10	25	13	22	6
	12	23	11	24	4
	10	20	7	5	5
	9	19	14	5	6
Outside.	14	19	11	16	4
Spider No. 160,	12	23	14	26	4
	14	25	16	27	6
	13	21	14	27	6
	14	27	16	30	7
	12	26	16	28	8
	17	16	10	18	5
	11	20	14	26	4
	14	20	11	26	7
	14	23	14	24	4
Spider No. 164,	9	28	17	29	5
	12	22	18	28	6
	12	23	21	34	7
	14	27	19	27	8
	12	26	18	29	6
	14	20	13	26	7
	15	23	21	35	6
	14	26	19	34	7

Argiope transversa.

In field. No. of Spiders.	No. of Webs.		Supports	Radii	Outer Spirals above	Outer Spirals below	Inner spirals
18	52	Av.	13.71	27	16.16	28.44	8.1
		M.V.	2.54	6.27	1.66	6.09	1.13
		Ex.V.	8	17	3	8	3
			19	44	31	51	15

Inside. No. of Spiders.	No. of Webs.		Supports	Radii	Outer Spirals above	Outer Spirals below	Inner spirals
8	23	Av.	13.5	26	10.6	22	7.5
		M.V.	2.02	4.78	7.13	7.78	1.69
		Ex.V.	9	15	2	8	5
			18	35	29	38	12

Individual A. transversa.

	Supports	Radii	Outer Spirals above	Outer Spirals below	Inner spirals
Inside.	17	33	25	33	7
Spider No. 106,	15	35	19	44	11
	12	27	3	18	12
	14	27	4	23	8
	18	22	4	19	9

	Supports.	Radii.	Outer Spirals. above	Outer Spirals. below.	Inner spirals.
Outside. Spider No. 106,	15	20	22	10	9
	13	—	29	14	6
	19	33	37	23	7
Inside. Spider No. 23,	14	33	29	38	10
	9	30	14	30	9
	12	34	22	36	8
	13	32	22	33	10
	15	24	3	10	10
Outside. Spider No. 117,	18	32	18	20	11
	14	20	12	16	9
	12	22	9	11	9
	9	20	11	18	6
	13	19	5	8	5
	10	22	12	17	7
Outside. Spider No. 39,	9	17	5	8	5
	12	19	7	14	—
	12	21	10	14	8
	12	20	11	11	9
	15	22	9	10	8

Epeira Sclopetaria.

No. of Spiders.	No. of Webs.						
33	82	Av.	8.36	18.57	17.13	22.04	4.83
		M.V.	1.12	2.51	3.43	3.94	.86
		Ex.V.	5	11	7	14	3
			14	25	28	35	10

Individual Sclopetaria.

Spider No. 92,	7	17	8	14	4
	9	23	22	20	4
	8	20	16	28	—
	7	17	23	24	4
	8	23	28	27	4
	10	23	22	27	6
	8	16	15	14	4
	10	16	10	22	4
	9	15	21	28	4
	9	17	21	28	5
	11	20	24	32	5
	9	18	20	26	4
	10	16	19	23	4

Individual Epeira Strix.

Outside. Spider No. 2a,	13	21	19	35	8
	9	21	17	23	5
Spider No. 6a,	10	13	10	13	3
Inside. Spider No. 17,	9	20	28	43	6
	10	23	23	43	7
	10	28	20	29	7
	9	26	25	41	7
	10	22	11	22	6

Individual Epeira corticaria.

	Supports.	Radii.	Outer Spirals.		Inner
Outside.			above	below.	spirals.
Spider No. 4a,	16	19	13	25	6
Spider No. 5a,	7	25	18	20	7
	11	26	11	14	7
	11	26	15	18	8
Spider No. 1a,	15	22	22	42	9
	19	22	20	39	11
Individual Epeira patagiata.					
Inside.					
Spider No. 51,	8	17	25	33	5
	8	15	22	30	6
	7	16	14	22	5
	7	16	14	22	5
	10	19	21	26	7
	9	20	21	26	5
Individual Argriope riparia.					
Outside.					
Spider No. 2 (n. s.),	14	31	21	32	14
	16	26	13	34	—
	17	22	12	23	8
	20	23	8	24	8

In making up the above tables care has been taken to leave out a very few of the most irregular webs made by spiders either in-doors or out-of-doors. Such irregularities were clearly due to very bad weather or to abnormal physiological condition. The most important part of each table is the number of radii. The outer spirals above and below come next in value. The supports and the inner spirals were most difficult to count and are of least value. It is not at all difficult to see from the above tables that there is great variation in the number of parts of the spider's web. The M. V. rarely falls below ten per cent. of the average and much oftener is much larger. The statement often found that "spiders build their webs with almost mathematical precision" needs qualification. To be sure, the members of the same species build a web, at any rate, of *generic*, if not *specific* value, if we make the web our basis of classification. But this value depends on the general arrangement of the parts, the size of threads used, and the presence or absence of certain parts. It could not be determined from the number of parts alone. Rainbow has therefore rightly objected to making the web of taxonomic importance, especially those spun under other than normal conditions. There is too much variation. Changes probably both external and physiological demand, as we have seen in the concrete examples of the building of the web, changes of procedure from the individual. There is greater variation in the results obtained from webs spun in the laboratory than from those spun outside.

But the number of spiders is very different. We do not know but that this difference would be diminished by using a larger number of spiders. Again, on the inside, every bit of the web was usually torn away by me each evening. Outside, the spider did her own tearing down of the old web before building a new one. From what little I have observed of this tearing down the spider in many cases leaves intact the old foundation lines. Combining these conditions with the change from out-of-doors to in-doors the greater variation is not surprising.

The results for individual spiders show that the variation is in part an individual matter. It is due, in large part at least, to particular conditions which the individual in question must meet. That this factor of variability between successive instinctive acts of the same individual appears in all the important tables above is of great significance. This would seem to be a much better measure of variability than the variation found to obtain between a single instinctive act, performed but once by a single individual of the same species, for example, the bending over of a blade of grass to make a cocoon. Prof. Davis (15, p. 47) has proposed such a measure of the variability of instinct. Indeed, he has found such by measuring the length of the fold in the blade of grass which the unknown spider uses for its cocoon. His abstract I quote in full : "A series of 222 nests of another species of spider, which binds grass or sedge blades in a peculiar fashion to form boxes for the protection of its eggs, was exhibited and a preliminary report on the variations and their causes was presented. Marked individual differences, including several anomalous types, appear, which probably represent (apart from accidental variations due to mechanical conditions) both variations in instinct and ingenious 'accommodations' to unusual conditions. Variations in length of the regular 'modal' forms conform closely in their distribution to the normal curve of frequency. In spite of the undoubted presence, in this case, of numerous factors which it is difficult to eliminate, it is thought that an objective measure of the variability of instincts may be possible."

How much of this variation, one would like to ask, is due to the size and age of this unknown spider or the difference in the width and thickness of the blade of grass? That some of the variability shown in the above tables and in other portions of this paper is due in part to similar causes I have no doubt. This will be made clearer by the curves given below. Until we can get, however, successive products of the same instinctive act by the same individual, and prove that such give us variation, there is not much progress to be hoped for in arriving at a satisfactory quantitative measure of the variability of instinct. At any rate, careful observation of the behavior of the spider in the performance of these instinctive acts is very desirable. This has not yet been done for the making of the cocoon above referred to.

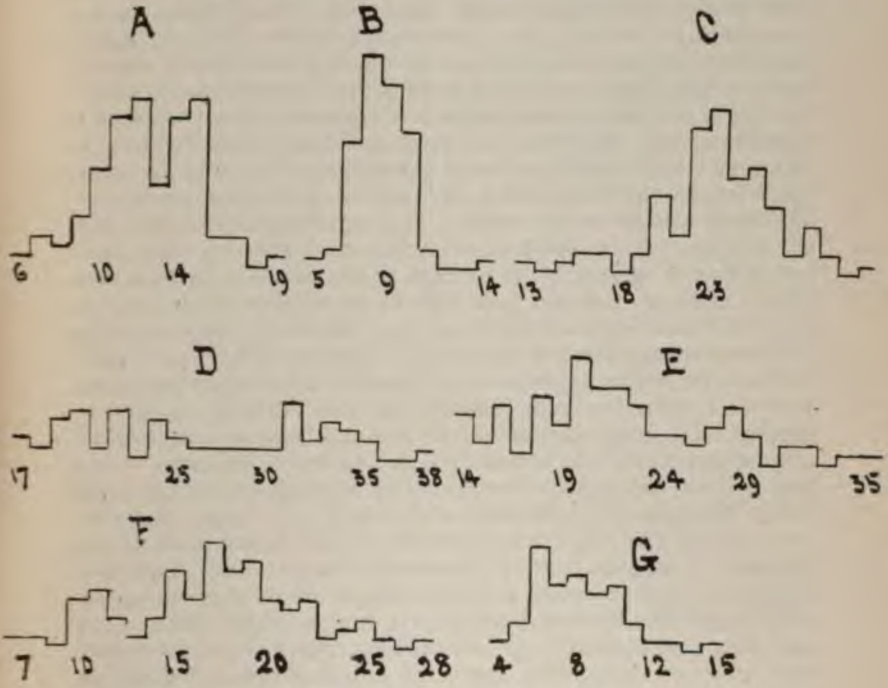

FIG. 7.

A represents the distribution of the number of *supports* in 90 webs of 20 *E. trifolium* spiders in the field; B the same of 82 webs of 33 *E. sclepetaria;* C of the radii of the *E. trifolium* in A; D of the radii of 52 webs of 18 *A. transversa* in the field; E of the Outer Spirals (below) of the *E. sclopetaria* in B; F of the Outer Spirals (above) of the same webs as in E; and G of the distribution of the number of Inner Spirals of the *A. transversa* in D.

In Fig 7, A, B, C, D, E, F, G, will be found a graphic representation of the distribution of the number of elements in the parts of the web by the method of surface frequency. The number of elements is represented on the base line in inches. The number of times this number of parts occurs in the webs is indicated by distances in inches above the base line. Only a few of the diagrams that could be made from my data will be represented here. The terms used in connection with each curve are made intelligible by the preceding tables. The above curves show more clearly than the tables which precede the great amount of variation to be found in the webs of spiders The supports and inner spirals vary within much narrower limits. If we had cases enough it is possible that these curves would all more closely approximate the curve of normal probability for mental traits. They might not show what some of them seem now to show. For example, in D, representing the radii of *A. transversa* in the field, there are really two curves, that is, the curve which we should expect for an animal that is at the present time developing two separate types of habit in this respect. However, as separate calculation shows, these separate rises in the curve are due to the fact that I have combined two separate counts; one, the first made late in the season, and the second made the next year but much earlier in the season. It is probable, then, that the age and size of the spider, the season, and possibly other factors, make it advisable to control the conditions, if possible, a little more carefully, at least to take cognizance of their presence, in the interpretation of results. However, the curves do not make it impossible that, so far as parts of the web are concerned, new habits might arise. As pointed out on pp. 310, 314, both this variation and, indeed, the possibility of the development by a single species of two types of habit are not totally new suggestions. Emerton (20) in a number of places in his recent book states that the number of radii for certain species varies between given limits. McCook (35, I, p. 107 ff.) also has counted and measured the webs of a number of different species. This same writer also states that certain *E. triaranea* and other species often spin threads across the open sector usually left free by this species. He suggests that here there may be a development into two different types of habit. However, no one (except Prof. Davis, as indicated above,) so far as the writer knows, has undertaken by this or any other means to obtain a quantitative measure of the variability of instinct.

In this connection occurs the very interesting, if at present insoluble, question as to whether function precedes structure in development. The above facts suggest that change of function may lead the way in the development of new structures or species. The nervous system being the most plastic tissue in the body might naturally be expected to lead in those accidental or other changes that give an opportunity for the working of natural selection.

The Time of Spinning the Web and the Stimulus which Sets the Web-spinning Act Going.

It was not long after my counting of the parts of the web began that I was curious to know just when the webs were spun, especially by the *Argiope* and *E. trifolium*. I often visited these spiders in the laboratory in the evening and first part of the night, but found that they did no more than move about a little. Two *transversas* which were in the field did nothing during the first part of the night. This was not true for *E. sclopetaria* and *angulata*, the young of which spin at most any time of day, while the older ones of these species usually spin at nightfall.

As a result of the above observations I began to watch at three A. M. On the first morning Nos. 106, 105, and 119, all *transversas*, began to move at about the same time. Nos. 98 and 99, *E. sclopetaria*, moved about, although they had spun webs at four P. M. on the day previous. This was at 5.30 A. M. about the middle of October. On the following morning the same reaction to the first appearance of dawn was noted. One 16-candle power incandescent lamp was left burning some twenty feet distant. This may account for the fact that No. 106 began before 5 A. M. on two later occasions.

On. Aug. 17th of last year I visited eleven *A. riparia* in the field at 3.30 A. M., and found all but two with the webs about two-thirds finished. These two had apparently just finished spinning. This was an agreement that I had hardly looked for. Again, in the month of September, I visited other webs at 5.30 A. M. and found one *E. trifolium* and three *A. transversa* with webs not quite finished. Prior to this I had noted that an early morning rain had stopped several *A. riparia* at about the same stage of progress in the making of their webs.

Most of the above facts seem to suggest that a common stimulus which sets them spinning is the amount of light. It is hoped in the future to determine this matter by actual experiment. With *A. transversa* and *A. riparia* this would seem to be when dawn begins to break, with *E. sclopetaria* and *E. angulata* when twilight gives way to darkness or perhaps in the morning as well. I tore away the webs of Nos. 98 and 99 at about the time in the evening for them to build if they were going to do so. They spun the next morning with the *transversas*. At any rate, so far as this study has gone, there seems to be a pretty definite reaction in this regard. Of course, one cannot be absolutely certain that light was the stimulus, but the coincidences point in that direction. Changes in weather and other conditions modify this reaction.

The above agrees well with what Prof. Emerton (20, p. 161) says of the three House *Epeirae* of which *E. sclopetaria* is one. "The webs are made usually at nightfall, very young individuals beginning to spin soon after sunset and larger ones beginning later, those that are full grown often waiting until dark, but some of them will occasionally spin their webs at any time of day."

We must conclude, then, so far as evidence goes, that the change from darkness to light and *vice versa* is the stimulus which usually sets the act of spinning going. The time at which the web is made would seem to have some significance in the life of the spider. I have seen *sclopetaria* remain undisturbed in her nest when her web was filled with flies. She and her close relatives probably feed at night, and hence the making of a fresh web at nightfall. Just what causes some or most of the young of this species to settle upon this time as they grow older, and in how far they do so, is a very interesting point which awaits further study. The *Argiope* and *E. trifolium* probably feed mostly in the daytime and on day-flying insects. Hence the importance of having a fresh sticky web with which to begin the day's work. If made the previous evening the web would be a much less efficient instrument for ensnaring prey.

In this connection may be mentioned the place which the spider is said to hold as a weather prophet. Büchner (7 a, p. 317) says: "When a storm threatens, the spider, which is very economical with its valuable spinning material, spins no web, for it knows that the storm will tear it in pieces and waste its pains, and it also does not mend a web which has been torn. If it is seen spinning or mending, on the other hand, fine weather may generally be reckoned on, so that spiders have long served as weather prophets." However, the observation of the Peckham's (41, p. 383) led them to write as follows: "After having observed spider after spider building a new web on the eve of a storm, how shall we explain the statement (which I have just quoted from Büchner). This would be, no doubt, the wisest way for spiders to act under the circumstances and Dr. Büchner is in very illustrious company when he—unconsciously, of course—orders the actions of such simple creatures in full accord with the higher reason." My observations agree perfectly with those of the writers last quoted. The reader will recall that Menge's conclusion as stated on page 309 is in agreement with this. I have seen any number of webs the making of which was stopped by a storm. Again, I have seen giant *riparia* webs, two and one-half feet across, spoiled by a rain only a short time after they were completed. In late autumn No. 39, *A. transversa*, spun while it was raining and sleeting. Facts such as these make it impossible to believe that these species are good prognosticators of the weather.

THE FEEDING HABITS AND INTELLIGENCE OF SPIDERS.

The feeding habits of spiders have been studied both in-doors and out-of-doors. Insects, grasshoppers, flies and bees were caught and placed on the web. Observations were also made on the behavior of spiders which caught their own prey in a perfectly natural manner. Notes were made at the time the spiders were feeding. In what follows the emphasis will be placed on the variations found to occur in the several acts which together constitute the feeding habits or instincts rather than on the identities which are, of course, of easy demonstration.

As pointed out above all spiders are voracious eaters. This explains their noticeably rapid growth, as well as the great variation in size in spiders which evidently have come from the same cocoon. Since, as in the case of *A. riparia*, and *transversa*, the young hatch in winter and do not come out of the cocoon till May, it is possible that there is a by no means insignificant struggle for existence while the young are in the cocoon so many months together; and the suggestion of Wilder and Lécaillon that the young eat each other is not at all an impossible one. This would certainly explain the marked differences in degree of development which are found. *E. sclopetaria* that live near a door to a house where there is an abundance of flies may become as large as a full grown *E. trifolium* while others, certainly as old, may be quite small.

The spider's first task is to detect the presence of prey in the web. To do this *A. riparia* and *transversa* stand at all times in the centre of the web on the under side with the eight feet carefully placed on as many radii, these being drawn a little taut by the spider, Fig. 1. *E. trifolium* and the house spiders usually stay in the nest with head pointing downward and with the first or second claw on either side holding a thread which is attached to the centre of the web, Figs. 4 and 5. This thread is spun last as the spider returns to her nest and is free from everything except the centre of the web and the spider's claw. It is a sort of signal thread. The House spiders may place their feet on the ordinary rays. When an insect strikes the web none of these spiders, as a rule, rush headlong after it. The *Argiope* may often be seen to draw the rays just a little tighter. *E. trifolium* and the House spiders start just a little out of the nest. It is as if they needed first to be awakened. They may stop several times before they reach the centre, presumably to see if their own movement has shaken the prey out of the web. At the centre they try, by halting a longer or shorter time, to locate the exact place where the insect may be found. That the centre is a better place for locating prey than any other in the web is shown by the fact that spiders often go the centre when a much shorter way is open to them. Boys (6, p. 149) has shown experimentally with the tuning fork this value of the centre for purposes ot orientation. They may also halt after they have left the centre. It is very interesting to see the spider during one of these halts suddenly jerk on the rays several times and then apparently wait for the stir which this should cause the insect to make. Whether or not the spider is then in an expectant state of attention is at present impossible to determine. Though usually so, spiders are not always infallible in locating their prey. They sometimes go four or five centimeters too far to one side.

The *Argiope* often do not wrap flies until they have carried them back to the centre. They may treat them as they always do a grasshopper—wrap them without first biting and poisoning them. Individuals of *E. trifolium* have always wrapped first except in one case in which I observed No. 170 catch a fly in her mandibles and not wrap until a strong wind apparently compelled her to do so. *E. trifolium*, *E. sclopetaria* and *strix* after wrapping usually carry the insect up into the nest, but there are exceptions to this. Before *E. trifolium* carries the food to the nest she, like the *Argiope*, returns to the centre of the web. Spiders generally spin a thread after them wherever they go, that at any time they may retrace their steps. The *Argiope* are usually anchored to the centre of the web so that they are prepared at once to spin their threads on the sudden appearance of food. These threads are often made much stronger than a single one by a spreading of the spinnerets. But in wrapping the insect this thread connecting them with the centre is lost and a new one must be spun; hence the trip back to the centre. I have seen *E. trifolium* spin as many as three of these threads back to the centre. This is, without question, an adaptation to enable her to care for an extra large insect. Nos. 106 and 105 have returned to the centre two or three times when the insect was an especially difficult one to deal with.

Nos. 14 and 106 showed caution at times in wrapping grasshoppers. Caution was also evident in the actions of No. 24, a very large *trifolium*, when she was trying to get over the web to her food. The single *E. strix* which I had in the laboratory had been fed on flies until one day I gave her a grasshopper. This led to some interesting actions on her part, as there seemed to be an element of surprise in her behavior. Before she reached the centre she stopped to jerk the rays two or three times. At the centre there was a halt for an instant. After leaving the centre to go down she stopped three times, giving to the radii several quick jerks each time. After reaching the grasshopper she tapped it some fifteen or more times before climbing on its back to search for a vulnerable spot. Finding it where spiders often find it, between the abdomen and thorax, she at once began to poison her victim. It does not take long for this poison to take effect, as the grasshopper ceased to struggle within less than two minutes. The web of *E. strix* is not suited to holding strong insects like a grasshopper, as I have observed out-of-doors. McCook's observation, that she is nocturnal in habits, suggests the same thing. It may be that this was a new food to her; she certainly acted as if it were. I tried feeding her grasshoppers after this, but could never get them ensnared in the web. They always escaped before she reached them.

A. transversa No. 23 seemed to show quick adaptation to a sudden change in circumstances when a blow fly was placed in her web. Just as she was on the point of reaching the fly it suddenly tore loose from the web. In an instant she was after it and had spun at the same time a wide strand behind her, the latter evidently because she was, as it were, already in the act of wrapping the insect. Fig. 6, page 325, shows the path she followed in catching the fly and the wide strand with which she anchored it to the centre. This is, at least, a rather extraordinary occurrence to which the spider did not fail successfully to adapt herself. We might interpret this as proof that the Orb-weaver can become an active and hunting spider, though I am not inclined to make so much out of it. Earlier writers have recorded observations seeming to show that an Orb-weaver losing a number of its legs took on the roving habit and attempted to secure its food in this way. Heineken and others, according to McCook (35, I, p. 78), have thrown considerable doubt on this point by removing, in some cases, all but three of the spider's legs and yet finding no change in the habits. A number of spiders have come under my observation which had suffered the loss of one, two, and even three legs. An example of the last I had under observation for three weeks. She showed no disposition to become a rover, spun a normal web, or fairly so, caught insects as well as a normal spider and received in a hostile manner the attention of a six-legged male. She was certainly hostile enough toward him to have been the cause of his deformity.

No. 14, *A. transversa*, treated some of the flies fed to her in a manner peculiar to herself. The instant she seized the fly in her jaws she let go and swung free, but at once climbed the thread she was suspended by, and was back at the centre where she wrapped her prey. This would do for small prey and was a matter of economy; for this procedure would no doubt save the web from being needlessly torn.

The spider has been said to show intelligence in the treatment of objects that fall into its web. Such, for example, as the cutting out of pieces of rotten wood, large insects, etc. I have tried tossing pieces of the hairy seeded spike of foxtail grass into the web of *riparia*. She wrapped the first one thrown in but on feeling of it more carefully cut it out, and let it drop, seeming to save as much of the wrapping as possible. After a short interval another piece was tossed in. She did not wrap this but merely cut it out, treating several more in the same way. After a time she paid no attention whatever to the pieces.

Was this an indication that she very readily detected the sham, modified her behavior, and later ignored it altogether? Does this behavior transcend the possibilities of instinct alone? Dahl's experiments, the results of which are given above, led him to conclude that spiders infer from analogy as well as profit by experience; also that they have a memory lasting several hours. But these experiments are almost all identical with Morgan's classical chick experiment and certainly need imply nothing more than ability to profit by experience. In many of my feeding experiments I have found spiders very uncertain quantities. They refuse to act when, to the human observer, there is no assignable reason for such a refusal. My attempts to corroborate Dahl's results (14, p. 173) are, so far, too few to allow of any generalizations. One *E. trifolium* would come no nearer than an inch and a half to the grasshopper which had been dipped in turpentine. The odor must have reached her from this distance, and I concluded that the solution was too strong. The Peckhams (41, pp. 393, 394) also found some evidence of ability to modify the "feigning instinct," and some proof of memory. Montgomery (38, p. 84) thinks that a male spider is just a little more wary in his attention to a second female if he has been treated roughly by the first just a short time before. From the results obtained by these writers and the few of my own which bear upon this point it is probable that spiders can learn and retain for a short time what they have learned. It is certainly premature to conclude that they infer from analogy. Further experiments are a great desideratum, however, in order that we may know how their intelligence compares with that of ants, bees, wasps, and the lower vertebrates.

THE WEB-SHAKING INSTINCT.

One of the most curious of the reactions to be found in two of the species of spiders dealt with in the present study is the instinct to shake the web. The Peckhams (41, p. 411) have noted something like it with *Epeira strix*. "Number one shook her web with sharp jerks when a branch to which it was attached was moved; and did the same when she was lightly struck." I have seen *Cyclosa turbinata* do this when her web was jarred by near approach to it. In one case only was I certain that *E. sclopetaria* shook her web. The amplitude of this vibration may be, for the centre of the large webs, at least four or five inches. This is not at all unlike the rhythmic swing which the boys like to give to the long foot-bridges across streams to the consternation of their weaker or more unfortunately placed companions. This is the only case with this species of which I am sure. It is true that this jerking may be analogous only to that which is to be seen in many spiders, when they shake the web, when objects fall into it. They seem to try to dislodge the foreign object by jerking sharply on the rays. And this may not be the same as the slow regular vibration which *riparia* and *transversa* give to their webs. At any rate, this may be brought about by bringing some object into the spider's range of vision which, if she is otherwise undisturbed, seems to be about one-half an inch. Other disturbances may also cause it. The spider may remain in the centre and shake the web or run to an upper corner shaking the web as she goes.

The usefulness of this instinct is not far to seek. Any enemy that is in pursuit over the web is placed in close straits in order to avoid the sticky concentric or parallel threads; or what is better still any large insect, a grasshopper, for example, which has only a partial lodgment and would do the web great harm if allowed to kick undisturbed, is aided in his efforts to escape.

VISION IN SPIDERS.

This section follows immediately that on the instinct of shaking the web because it was the manifestation of this latter that led me to observe and test the sense of sight in spiders. As I have already said, the object, the sight of which is to induce the vibration must be brought very near to the spider. But I soon observed that after the vibration was started the object need not be brought at all so near in order at once to cause the spider to increase the amplitude of the vibration. After this increase subsided another test could be made with the same results. Many tests have been made in this way. At least I am convinced that to awaken the spider, so to speak, with a preliminary disturbance is sufficient to increase her range of vision, at least, some six or eight times. This was for a piece of white card-board one inch square and fastened to the end of a brass wire so that it might be handled conveniently. With *riparia* I have noted that a movement of my body two and one-half feet away was followed by an increase of vibration. Further experimentation is necessary before I can be absolutely certain that jars were entirely eliminated. I have thought that clothes light in color and moving objects were detected when farther away than darker and fixed objects. Of course, if the spider is tested while she is shaking the web this is equivalent to making the object move.

Another sign which they give on seeing an object consists in elevating slightly the posterior end of the abdomen. The distance which is sufficient to produce this is about one inch. This would seem to be about the normal limit of vision for *E. trifolium* so far as I have been able to test it. One of the latter species failed to see another individual of her own species, which was made to invade her nest, until very near. *Agelena naevia* did not spring for flies until they were about the same distance from her. I have, nevertheless, seen a *Phidippus tripunctatus* jump a distance of fully an inch and a half and catch another spider, *Tetragnatha laboriosa*, which was eating a fly in her own web.

The distance given above for *Argiope*, at which they seem to detect the presence of objects is not by any means unheard of. Hentz and Bingley have both given to spiders a very keen sense of sight. The Peckhams (41, p. 402) on the basis of their observations of both males and females make the following statement: "The ocelli of some spiders, then, enable them to see objects at a distance of at least ten inches." Forel (24), however, calls a fly stupid that could not escape from a spider, but he probably underestimates vision in spiders.

The spiders of the authors just quoted are perhaps the more active or hunting spiders. It is not asserted here that the large and sluggish *Argiope* can do more than detect the presence of an object at a distance of six to ten inches. It would be quite another thing for them to distinguish a fly or another small spider as prey and spring upon it at this distance. But the question may very well be asked: Would it not be rational to suppose that, after being made to vibrate once and thus put on the alert, the spider should be able to see farther and better and thus at any rate detect the mere presence of a large and light colored object? It will be recalled that Dahl gave spiders both a far and near range of vision although their eyes consist of eight of the simple ocelli of insects and have little or no power of accommodation.

It may be, however, that all we have in the above is a proof that attentive vision in spiders is more effective than inattentive vision. As suggested in the section on Feeding Habits, there is some proof that the spider must be first awakened before she starts off to catch and wrap an ensnared insect. These results on vision would indicate all the more that the spider is in a more or less sleepy condition while she is in the nest or standing in the middle of the web. The spider's manner of life is such that we might expect it to require periods of profound rest. The making of the complex web in from three-quarters of an hour to two hours, as well as the wrapping of prey, demands the expenditure of an extraordinary amount of energy in a comparatively short time. It would seem best, then, to regard the above facts as most satisfactorily explained by greater alertness.

THE MATING INSTINCT IN ARGIOPE.

This instinct is one of the most interesting in spiders, or for that matter in the whole animal kingdom, and this is especially true when the subject is approached from a psychological point of view. Some of the difficult questions that press for answer may be stated as follows: What starts the male in his search for the female and does he search for her at random or is he guided by some sense or senses susceptible to special stimuli? Do males of closely allied species ever get on the webs belonging to other species? Are there definite steps in the male's approach to the female? How far is it true that there is a battle for supremacy among the males? Does the female exercise any choice among the males? These and other questions are suggested in the investigation of this instinct.

In what follows I shall not try to solve any of these, but merely offer a few facts gathered in an incidental way on some of the questions raised. My observations are confined, so far, to two species, *A. transversa* and *riparia*. The male of *riparia* was first observed on the edge of the web, or on the side-screens to the web, of the female. In a few instances the male made a very imperfect web of his own. The next step showed the male above the female at the upper edge of the web, and on the same side. From here he gradually went down toward the female and often stood for hours an inch or so distant. Next the male spent several hours, in one case a day or more, on the opposite side of the web and very close to the female. With the male in any of these positions one could easily see that he was keenly alive to every movement of the female. If the wind shook the web so as to make it necessary for her to readjust herself, the male followed her with a readjustment of his own. Just before she moulted the male again took his position on the same side of the web and immediately above her. See Fig. 8. This moulting takes a variable time with different females. My observations as to the manner of moulting agree perfectly with those of Montgomery (38, p. 145). He says "the moult in all spiders follows the same plan: a horizontal split of the old skin along the side of the abdomen and of the cephalothorax (here just above the leg and the jaws), so that the skin breaks into a dorsal and ventral piece. This is quite different from the process of moult in insects and crustacea." This manner of

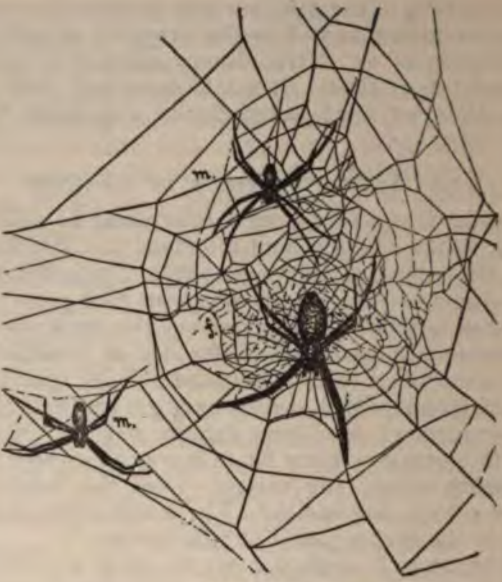

FIG. 8.

This figure represents the third main step in the approach of the male, M., to the female, F., in the mating of *Argiope transversa* (see text). It may also represent the end of the first step.

splitting horizontally is very well shown in Fig. 11 below. In one case the moulting and copulation was all done in four hours. In another, the female was beginning to moult in the morning at eight A. M., copulation began at 5.45 P. M., and the male was caught and wrapped by the female at 7.07 P. M. During the latter part of the moulting while the female's legs were about half out of the old skin, the male was constantly following the progress of the moulting. The moment she freed herself from the old skin he took the position shown in the accompanying drawing. She hung suspended by the wide strand of web which had exuded from the spinnerets while she was moulting and which is clearly shown as attached to the old moult, her legs all hanging limp. After a time she supported herself by taking hold of the threads of the side-guard of her web. After one hour and twenty-two minutes of copulation she suddenly brushed the male off and wrapped him as she would any other prey leaving him attached to the strand to which she had been hanging. She then took her position head down in the centre of her web. See Fig. 12, which shows the sequel of many acts of mating with the *Argiope*. The next morning conditions were unchanged except that she had cut

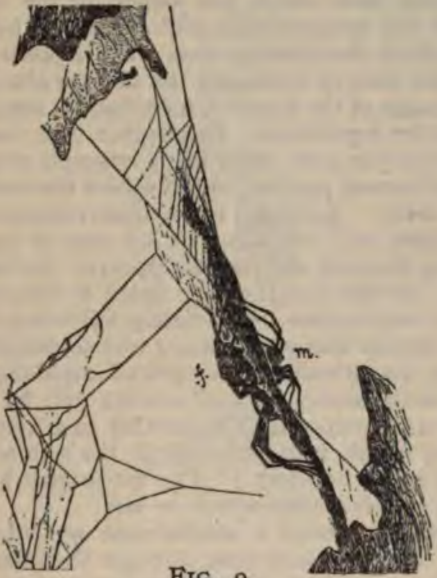

FIG. 9.

This figure shows the second step in the approach of the male toward the female in *Argiope riparia*. Any of these figures illustrating the mating of one of these two species illustrate that of the other as well.

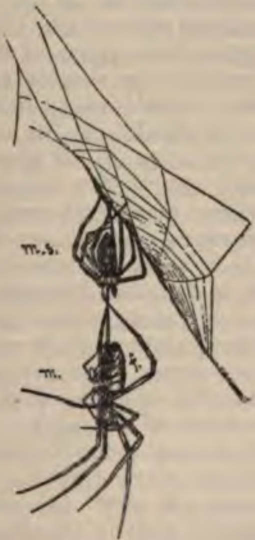

FIG. 10.

This figure represents the copulation of *Argiope riparia*. The female, F., has just dropped from the moulted skin, m. s.; m. is the male.

out the moulted skin which had lodged in the lower part of the web with the wrapped male still suspended from it.

While I believe the above process often happens, the death of the male is not always the sequel, nor does he always succeed in taking advantage of the female's moulting to secure the necessary position for copulation. In another case the female had moulted and yet the male made many attempts to approach her. At times she seemed passive, but at others she was hostile and drove him away. At night, when observations were discontinued, he rested with one fore foot on one of her hind feet. The following morning she had disappeared leaving him alive on the web. In this case the male failed to take advantage of the female's helplessness, immediately following moulting, or she failed to entrap him after coition was accomplished, and he continued in his attempts for repeated copulation. When I placed him on the sticky part of another web he was immediately caught and wrapped. (In another case I atoned for this act by freeing a male who had stuck to the web of *riparia*. He lost two legs in the accident.) The males must keep free from the adhesive spirals if they would be safe.

What must have been a similar case was observed in *A. transversa*. The male had gone through the same steps as for *riparia*. He was present at the moulting and may have succeeded in the act of coition for a few seconds during the moulting, but following this he did not again succeed in copulating although he made every effort to do so. Most of his efforts were confined to the dorsal side of the abdomen on the opposite side from the reproductive organs of the female. His palpi were repeatedly thrust into the skin of her back, or posterior part of the ventral side. The drawing below shows him in the act of doing this. After continuing his unsuccessful attempts for two hours, during the latter part of which he rested often, he seemed to give up altogether. The female was small and this was probably not her final moult. She was hostile whenever he shook the web. This made me curious to know if the female would ever be other than hostile if the male shook the web. Of course, while she is helpless through moulting it would make no difference. My observations point to the fact that the male not finding the female ready to moult leaves to return another day; also to his mating with more than a single female. It is quite possible that the moulting of the female is in part a response to the appearance of the male. Two hours later in the first case described above the female had cut out the moulted skin and the males were gone. One week later she was found dead in her web with her abdomen twisted over to one side. Whether the male in his attempted coition caused her death cannot certainly be known. Her manner of death is the only one of the kind I have observed.

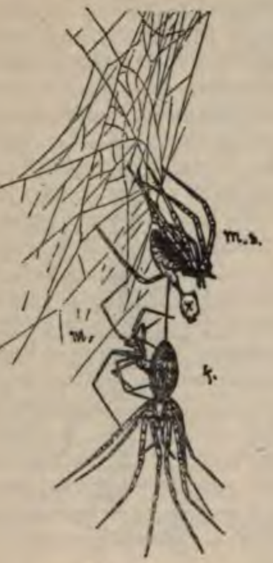

FIG. 11.

Fig. 11 represents an attempted copulation in *Argiope transversa*. The female has just moulted as in the preceding figure. The preceding steps in the two cases were much the same.

That nature has taken as careful precautions to insure the fertilization of the female spider as she has the queen bee and ant, though probably not in such a prodigal way, is proved by the fact that often there are two, or more, and sometimes even five, males on the web with one female. Fig. 8 shows but two of the three males which were present.

The above cases, which are the only complete examples of approach and copulation I have seen, prove that *riparia* and *transversa* are very similar here as in other respects. But with both I have followed the approach of the male for days only to find after a few hours' absence the moulted skin of the female and the wrapped male. In several cases the approach of the male was the same as in the completed acts observed. It is, at any rate, clear that the interpretation which McCook gave to his and Emerton's findings, which are represented in Fig. 12, is not the only one. Such do not always mean that the male has been too urgent in his attention to the female.

Let us return to our general questions. In the first place, it would seem improbable that the male is lead by sight in searching out the female. The distances are without doubt too great for his poor vision. Is it something akin to the antennal sense in ants and bees, of which Forel (25) and Miss

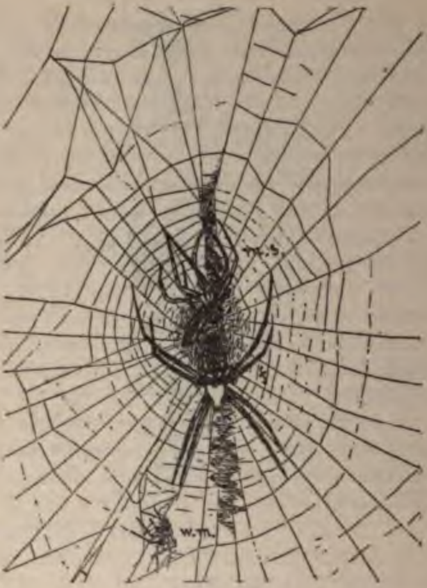

FIG. 12.

Fig. 12 shows the sequel to Fig. 10. After the female has sufficiently recovered from moulting, she wraps the male as shown in the lower part of the drawing and takes her usual position on the web.

Fielde (22) have given such good accounts? Judging from the few observations I have made, this seems improbable. A male of *A. transversa* who had conformed ostensibly to all the regular rules in paying his respects to the female, finally lost her, probably because both "feigned" and dropped to the ground at my approach. She came up about six inches distant from the old web and moulted without his presence. An hour or two later he began to wander about and his curve of search so far as I plotted it, seemed to be a wholly aimless one. It very much resembles those given by the Peckhams (41, p. 400) for spiders in search for their cocoons.

Again I found a male of *A. transversa*, with the third leg gone from the left side, on a web of *E. trifolium*. The next day he was suitor, No. 2, to a female of *A. transversa* some three feet away. Hence, it would seem quite certain that McCook's surmise that male spiders at times make mistakes is an actual fact. It also seems quite probable that males of *A. transversa* and *riparia* should make mistakes of the kind just mentioned since they are so closely allied and similar in habits. These examples rather point to the fact that male spiders find the webs of the female at random. Further progress might well be due to tactual and visual stimuli.

After he has located the female, what are the reasons for his fear of the female and his caution in approaching her? We have seen in the above account that he does manifest both caution and fear, and the typical approach in spiders would seem to be by a series of acts almost as formal as is to be found in the best human society. In the first place all solitary spiders instinctively separate from each other as soon as they leave the cocoon. The female spiders which I have placed on another's web, seem to be in great haste to get off. Can it be that the male has some of this instinctive fear of the female's web? Yet this fear and caution must give way at the right time to the sexual impulses which, of course, must be strong, in order that the male accomplish the one thing whereunto he is sent. The male must reach a high state of excitement before coition takes place. This, of course, is necessary to insure the best results of coition. Montgomery (38, pp. 135 and 142), to whose excellent article I owe much, holds that mating in spiders is more remarkable than in other animals because the male must charge, and perhaps recharge, the palpi with sperm. Lastly, the male must learn that the female is not hostile. This, in the spiders here considered, is indicated to him by her remaining quiet.

In the above statements do we find any support for believing that there is a choice by the female of the most beautiful or active males? The males on one web, Fig. 8, did fight for supremacy. In the case described above where the male did not succeed in copulating the two males on the side had an encounter. The one farthest away, or the one I had previously selected as the smallest, retreated. The victor, or second in size, then approached the largest one which happened at the moment to be resting. But the aggressor suddenly beat a hasty retreat, leaving the largest male to continue his attentions. The males of these species do not go through the antics or "showing-off" acts which are found in other species. They, however, do struggle for supremacy, and Montgomery (38, p. 144) very probably is right when he says that the female chooses that male which "first and most surely announces by his movements that he is a male." The largest and therefore earliest to develop, as I was able to select beforehand in the above example, would naturally do this.

Can we account for all the facts by natural instinct, pure and simple, or must intelligent adaptation on the part of the individual spider be appealed to? Given a physiological change in the male spider which is not without its analogy in the instinctive actions of other animals, for example, in the migration of birds and salmon, he begins his search for the female. Once on the web, at the edge or on the side screen, he next goes to the upper edge and then down to within about one inch of her, then on opposite side. If she feels for him at any time he retreats. Next he goes to the same side, and just about now the moulting, if it is going to take place, should be indicated by her flexed legs and body farther out from the web than is usual. If she moults, the male constantly follows the progress of this act. At once when her legs drop from the moult he is in position and coition begins. Montgomery (38, p. 135) has shown that this may last hours or seconds and may be repeated many times depending on the species. He has also (p. 125) emphasized the variation to be observed in some of his species. While the series of acts constituting mating must allow for some variation, the acts of both sexes, when conditions are favorable, appear to be, in the two species here considered, pretty definitely correlated with each other. Intelligent adaptation, to any great extent, is perhaps uncalled for in their explanation.

TROPISMS VERSUS PLASTICITY IN INSTINCT.

Are there any reactions in spiders which may be denominated tropisms? Almost from the very first I have been on the lookout for such reactions. For the present I have concluded that there are, at least, a few such which are unvarying enough to be classed under this head,—namely, that of standing in the web with head down, the way the *Epeirae* stand in the nest, their thygmotactic reaction to sides of the nest, the direction in which the guide lines or temporary spirals, and the outer permanent sticky spirals are put in. Figs. 1, and 8-12 inclusive, illustrate the first of these reactions; Figs. 4 and 5, the second. Standing in the centre of the web with head down is also true of the males of *Argiope*. The spiders that have nests hold approximately this same position much of the time. When they stand in the middle of the web they orient themselves in exactly the same way as the *Argiope*. It is true that in taking this position all are facing the lower part of the web, which, since it constitutes the larger half, allows them more readily to locate and wrap their prey. But we must suppose that gravity is one of the forces toward which they are reacting. This is always the same. Spiders also orient themselves unmistakably with reference to the centre of the web, both in the original construction of it and in the use of it later. Directions within the web are all the same when once the centre has been located. Indeed, if we enumerate the conditions to which spiders must adapt themselves, gravity and direction are the only ones which at once appeal to us as being constant. Yet it is evident that there are relatively few tropisms in spiders. It is surprising and almost incomprehensible how long we have held to the belief in the non-variableness of instincts. They must of necessity vary if the individual is to use them in living successfully in an environment that in many ways is one thing to-day and quite another to-morrow.

Do the many variations pointed out above prove that the spiders here concerned have intelligence? Not conclusively it seems to me. A few of the acts observed which are similar to those noticed by Dahl do point that way. But many of the variations fall under one or the other of the very suggestive classifications of Wagner (67) which are quoted with approval by Rainbow. These are first "deviations of instinct," and, second, "variations of instinct." The first are slight and have no value in the struggle for existence. The second are greater variations from the instinctive procedure of the species. They may be seized upon by evolution and affect the future development of the species. There are many examples of these in spiders. Such as the omission of the zigzag, or the "darning" of the centre, the putting in of more outer spirals above than below the centre, *E. trifolium*, *strix*, and *sclopetaria* standing in the centre of the web instead of the nest, the different methods of treating prey, etc.

To emphasize further the place which plasticity should hold in our scheme of explanation of animal activities it is only necessary to refer to Prof. Forel's recent publications (24, pp. 562 and 563). He writes in substance as follows: In our study of insect psychology we must avoid two evils, (1) that of identifying our mind with that of the animal, and (2) of imagining that with our present knowledge we are able to construct the insect mind. On the contrary we must recognize that this insect mind and the senses, which when functioning awaken it, are derived from the primitive life of protoplasm. This life, so far as it is characterized by irritability of the nervous system and connected with a contractile muscular system, manifests itself in two ways. First, as automatic or instinctive activity and, secondly, as plastic or intelligent activity. The former is more or less adapted to the circumstances of the specialized life of a variety, species, or genus. It is insidiously analogous to the action of a machine. But nearly all the recent work in Animal Psychology clearly shows that those very animals from the reactions of which the adherents of the theory that animals are "reflex machines," get their best examples, are, in reality, able to modify their actions, use a method of "trial and error," and do have plasticity.

Any attempt to divide activities into Reflex, Instinctive and Voluntary does as much injustice to real life in the animal series (as it is hoped the many observations and facts of spider life given above will help to prove), as it does to divide the human mind according to the old faculty psychology. This seems to me doubly true of the higher invertebrates such as the ant, bee, wasp, and spider. Weismann (70a, p. 162) says, "A sharp line of distinction between either Reflexes and Instincts, or between the latter and voluntary acts, is not to be made. They shade over into each other and this is quite equivalent to saying that in plyletic development transitions from one form of activity to another have occurred." If the above is true it follows that a "deviation or variation of instinct" may very well offer a basis for the development of intelligence.

As Miss Washburn (69) has already pointed out, Professor Royce, in his "Outlines of Psychology," has done inestimable service to Psychology by the manner in which he has made the results of Animal Psychology contribute both to the view point and manner of treatment of his subject matter. His use of the Tropisms is peculiarly suggestive. He says (56, pp. 142-143) "The researches of Loeb and others have called attention, in the recent literature of genetic psychology, to the vast importance which is possessed, in all grades of animal life, by the types of reaction which have been called the tropism of Orientation. We earlier made mention of such reactions when we were speaking of the various tropisms which Loeb has experimentally examined, as they exist in lower organisms. The general character of such reactions is that they determine, in an organism of a given type, a certain characteristic normal position of the organism with reference to its environment, and certain equally characteristic tendencies on the part of the organism to recover its normal position when it is for any reason temporarily lost, and to assume, in the presence of stimuli of certain types, certain directions of movement and certain attitudes which may persist through a great variety of special activities. The phenomena here in question are, in a sense, very familiar to us all. The animal laid upon its back may struggle back again to the normal position. Or again, the human being when engaged in normal activities either sits or stands erect. When the eyes are engaged in their normal activity, the head is held erect, or, if these normal attitudes are modified, as in reading or in writing, the modification occurs only within certain limits. To attempt to carry on the same activities when lying on one's back, leads to discomfort, and interferes with the normal special movement of the eyes. It is thus a familiar fact that a certain orientation of body, that is, a certain general direction of the organism with reference to the most important kinds of stimulation which are falling upon it, is a condition prior to all special activities. Hence the reactions of orientation are amongst the most fundamental phenomena of healthy life. Profound disturbances of orientation necessarily imply very considerable defects and in most cases very gravely important defects, in central functions. Thus our responses to our environment are not only special deeds, such as grasping this object, or looking at that object, but include general attitudes, namely, such acts as sitting or standing erect or holding the head up in order that we may see. And the special acts are always superposed upon the general acts, in such wise that if the general tropisms of orientation are seriously disturbed, the special acts, however habitual, will be interfered with or will prove to be impossible."

Further on in the discussion of Active Attention (pp. 329-330) the same author says, "In brief, whoever is persistently attentive is expressing an attitude of the organism which has the essential character of the now frequently mentioned 'tropisms' of the animals of Loeb's experiments."

As will be evident from what has been said above of "tropisms" in spiders, this creature furnishes us with an excellent example of the fundamental tropisms of orientation and also of direction. While in a few cases these are like the tropisms in many other animal forms, they are more adaptive. The spider is capable of carrying on other and special activities without exact orientation, though it is true that she would feel more at home if normally oriented. Professor Royce's view that the "tropisms" are the basis of Attention, even of Active Attention, seems to me possible only on the assumption that instinctive activities suffer variations such as may be seized upon by natural selection. It is hoped that the present paper will help to show that the life of spiders furnishes us with some proofs of this.

SUMMARY.

1. The spiders whose names are given on page 314 show wide variation in color markings and degree of development. They also show great variation in selecting a place for a web and, for those that build one, the nest and the choice of material for the same.

2. In making the web they seem to be able to adapt themselves to the peculiarities of the place or supports for the web. They may make unusually strong any part of the web if external conditions require it. A change by the spider to a very different place has led to the greatest variation.

3. The webs vary from the normal slant, the upper outer spirals may be more numerous than the lower ones, which in a way is equivalent to turning the web upside down. Those that put in a zigzag or "winding stair" may in many cases put in only the lower half of it, or omit it altogether and the centre

may be left undarned. When a spider has the same branched twig in which to sling her web, she gives to almost each new web a different slant.

4. As for the number of parts it will be readily seen from the tables that these vary within limits. The variation among members of the same species as the "mean variation" shows is too great to be the product of an instinct followed with mathematical precision. In the case of successive webs of the same individual the same variation is found. The results obtained for the group cannot be due, therefore, to differences between individuals only. Since all of the Orb-weavers have the same sorts of changes in conditions to meet it is to be expected that their variations would be of the same kind and degree. Such is actually the case—*Argiope transversa*, *Epeira trifolium*, and *Epeira sclopetaria* showing a striking agreement in both specific and individual variation. The counts for individual *Epeira strix*, *Epeira patagiata*, *Argiope riparia*, and *Epeira corticaria* show the same thing.

5. The young spiders, especially of *E. sclopetaria*, have been watched spinning their webs and some of these have been counted. They probably choose the site for their web in the same way as the old. They certainly make the web as rapidly and they are really more perfect since the webs are so small that a large enough niche can be easily found and the weight of the spider does not bend the threads so that they stick together. I have also seen webs of the young of *A. riparia*. They are as perfect as those of the old, if not more so. Young of *E. angulata* have been seen spinning the web and they were as adept as the old.

6. The feeding habits of spiders show powers of modification that seem to be intelligent. Dahl's experiments, as also those of the Peckhams, point to the fact that spiders are able to profit by experience, and have a memory lasting a few hours. Such of my tests and observations as would prove this for the spiders here dealt with are inconclusive. *E. strix*, *E. sclopetaria*, and *A. transversa* use different methods with different prey. *A. riparia* refused after a first trial to wrap what is not food and later ignored its presence in the web.

7. The time of spinning of the web depends on the species. It is very probable that *A. transversa*, *A. riparia* and *E. trifolium* spin their webs at the first appearance of light in the morning. Old *E. sclopetaria* and *E. angulata* spin at nightfall. Young of both the latter spin at almost any time of day. *E. sclopetaria* move about at early dawn and may spin then if they have not spun the evening before. The reaction to light here is a pretty definite one.

8. The web-shaking instinct is most marked in the *Argiope*.

In fact, only one case approaching the slow rhythmic swings which they give to the web has been observed in the other species and that in *E. sclopetaria*. It is brought about by touching or otherwise disturbing the spider.

9. Tests with these spiders in a state of rest and after they have been made to shake the web, or have otherwise been put on the alert, show that they have a range of distinct vision, at least six to eight times that of clear vision under ordinary circumstances, which is about two centimeters. It may be more for very large and light colored objects.

10. The approach of the male in the mating of the *Argiope* is probably divided into a number of fairly definite steps. The male seems to hunt at random until he finds the web of the female. He then approaches her from the upper edge of the web on the same side. Next he approaches on the opposite side of the web and remains just opposite to her for some time. He then returns to her side of the web and takes his place immediately above her. If she moults he keeps close watch of her progress. As soon as she drops from the old skin, coition occurs. This may last a variable time after which the male is wrapped and dies. If the female is hostile and not ready to moult the male may leave the web. The above steps are not invariable.

12. The variations which are recorded in the foregoing sections are convincing proof that instinctive activities in spiders are variable, and it is easy to find teleological reasons for such variability. These variations are so marked in some cases that they may well be seized upon by natural selection and made starting points in the development of new types of habit, if not of new species. That they furnish a basis for intelligent behavior in these spiders is probable, though the extent to which this sort of behavior has been actually developed can be affirmed only after further study. This point and others the writer hopes to follow in later researches.

It is a pleasure to acknowledge my indebtedness to Pres. G. Stanley Hall and Prof. E. C. Sanford for suggesting this topic of research and for assistance in studying it, and to Dr. M. T. Thompson, my colleague, for the excellent drawings with which I have been able to illustrate this article, as well as for one of my most valuable spiders, No. 23, and for many helpful suggestions. Prof. Emerton, of the Boston Society of Natural History, has kindly identified all my specimens for me and has helped me much with his interest and enthusiasm. He sent the *Phidippus*, referred to on page 340, to Prof. Peckham who kindly identified it specifically. Lastly I am very much indebted to the librarians of the Clark and Harvard Libraries for assistance in getting at the scattered literature of the subject.

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A STUDY OF THE AFFECTIVE QUALITIES.

I. The Tridimensional Theory of Feeling.[1]

By Samuel Perkins Hayes, A. M., Ph. D.

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Introduction.

Wundt's theory of feeling has undergone a continuous process of development, in wnich three fairly distinct stages may be distinguished. In the first period "there is but one class of elements, sensations. Feeling is a third attribute of most of these, and represents their subjective aspect. More specifically, it is the mode of reaction of attention upon sensational content. The way in which a perception is taken up into consciousness at large determines feeling. Feeling thus depends on the general subjective disposition in a way which is not true of quality and intensity. Its poles are pleasantness and unpleasantness." In the second period, "quality and intensity of feeling are mentioned for the first time, and its close relation to will is emphasized. Pleasure and pain in the form of desire and repulsion govern all volition. Feeling is on the one hand inseparable from sensations and ideas, and on the other, it could not exist without a will, the tendency of which it manifests." In the third period, feelings become independent elements in consciousness: they cannot be reduced to any simpler elements; and, although we cannot isolate them directly as we can sensations, by focusing the attention upon them, we can isolate them indirectly—from each other—by isolation of their perceptual substrate.[2] It is in the writings of this third and final period that Wundt's tridimensional theory first appears.

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[1] From the Psychological Laboratory of Cornell University.

[2] Hollands: Wundt's Doctrine of Psychical Analysis: II, Feeling and Feeling-Analysis. *American Journal of Psychology*, XVII, 1906, 208 ff., 214.

The general outlines of the new theory are well known. Simple feelings are at once more numerous and more various than simple sensations; but they may be grouped in a tridimensional manifold, in which the various intensities of feeling are indicated by distances from the central indifference point, and the qualities of feeling by the direction from this indifferent centre. Our concrete feelings are, in the great majority of cases, compounded of all three feeling dimensions; an actual feeling that lies in one dimension only, or even in two dimensions, is an exceptional occurrence, a limiting case.[1] In other words, the simple feelings tend strongly to fuse; they give rise to unitary feeling resultants, which are introspectively unanalyzable as they occur. If we wish to analyze them, we must have recourse to the sort of psychophysical analysis that we employ, *e. g.*, in the case of a color sensation. By varying the components of the color stimulus, we discriminate color-tone, saturation and brightness, though the color as given is a quality of sensation, psychologically unanalyzable.[2] So with feeling: by varying the components of the perceptual substrate, or by varying the sensory stimulus in the case of the multidimensional sensation systems, we are able, psychophysically, to bring out the elementary components of the unitarily experienced feeling.[3]

The tridimensional theory first appeared in the *Grundriss der Psychologie*, 1896. In 1899 Titchener published a criticism of the theory as here formulated, maintaining that strain-relaxation and excitement-depression are not pairs of maximally opposite qualities, but rather positives and negatives; calling attention to the fact that Wundt's treatment of feeling in the *Vorlesungen* of 1897 was at variance with that of the *Grundriss;* and asserting that Wundt had presented no experimental evidence for his theory.[4] Wundt replied in detail to this criticism, and appealed to the curves in Lehmann's *Die körperlichen Aeusserungen psychischer Zustände* as experimental evidence.[5] Stevens then made a study of Lehmann's *Atlas*, and showed that the curves in question could not be regarded as evidence for the tridimensional theory.[6] Titchener next attempted to submit the question to experiment, by a variation of the method of impression, and published his results in the Wundt *Festschrift*.[1] In these experiments the evidence, so far as it goes, is decidedly in favor of the dual, and against the tridimensional theory of feeling.

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[1] Phys. Psych., II, 1902, 288, 307.

[2] See I. M. Bentley: *American Journal of Psychology*, XIV, 1903, 92 ff.

[3] On this point, and on the various changes in the exposition of the tridimensional theory (changes that may be summed up as showing an increased caution in statement, and a greater readiness to admit the possibility of feeling-analysis), see Hollands: *op. cit.*, esp. 218 f.

[4] Zeitschrift f. Pschol., XIX, 1899, 321 ff.

[5] Philos. Stud., XV, 1900, 149 ff.

[6] *American Journal of Psychol.*, XIV, 1903, 13 ff.

The evidence, however, does not go far enough. As the author himself points out, the number of experiments was limited, and they were made in two only, not in all three, of the Wundtian dimensions.[2] And this criticism has, naturally enough, been repeated by various reviewers.[3] The question then remains, whether or not it is worth while to extend the investigation; to increase the number of experiments, by calling in the aid of new observers, and to take into account, for one and the same set of stimuli, all three of the feeling directions. In order to answer this question, it is necessary to estimate the idea which underlay Titchener's study. That idea was two-fold; and the study appealed to two lines of evidence, objective and subjective. On the objective side, there is the appeal to the 'curves' which present in quantitative form the course of the affective judgments. No one denies the validity of pleasantness-unpleasantness as a feeling dimension: all other dimensions are matters of dispute. If, then, the resultant curves all take the form which is taken by the curves of pleasantness and unpleasantness,—if there is no specific type of curve for excitement-depression and strain-relaxation,—then we have at least an indication that pleasantness and unpleasantness are the only fundamental affective categories. Apodictic proof we most certainly have not; but the indications will be in favor of the dual theory. For it would surely be a strange thing if a given set of stimuli affected a given observer by way of excitement-depression (or strain-relaxation) precisely as it affected him by way of pleasantness-unpleasantness. Coincidence might occur here and there; but the wider the range of observers, the larger the number of stimuli employed, and the more varied the type of affective judgment, the less likely would it be, on the basis of the plural theory, that coincidence should appear. On the subjective side, again, there is the appeal to the introspection of the observers. If the observers declare that the affective judgment in terms of pleasantness-unpleasantness is direct, easy and natural, while judgment in terms of strain-relaxation and excitement-repression is forced, difficult, associatively mediated, etc., then the evidence of the method is in favor of the dual theory. And if, further, the observers state that their judgments of excitement depression and strain-relaxation, so far as they are affective judgments at all, are based upon pleasantness-unpleasantness, this evidence is proportionately strengthened. Here, also, the value of the evidence will be cumulative: the more numerous the observers, the more varied the stimuli, the more nearly exhaustive the affective categories, the more certain will the outcome be.

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[1] Philos. Stud., XX, 1902, 382 ff.

[2] "Freilich ist nicht zu übersehen, dass dies Resultat bei einer zweidimensionalen Darstellungsweise und der Begrenztheit der Versuche nicht beweisend ist." *Op. cit.*, 405.

[3] So, *e. g.*, Moskiewicz in Zeitschrift f. Psychol., XXXIV, 1904, 314.

Unless, then, the above reasoning can be seriously impugned,—unless, that is, some better method for the investigation of the tridimensional theory can be proposed,—it would seem very much worth while to extend the experiments of the Wundt *Festschrift.*[1]

We are now, therefore, confronted by the problem of the choice of stimuli. Experiments made by the method of paired comparisons consume so much time that selection is necessary; it is impossible, in a single investigation, to cover a very wide field. In the earlier study, clangs and metronome beats were chosen as offering, on Wundt's own testimony, salient examples of what we may call the unorthodox feeling dimensions. Whatever else they may be, Wundt said, clangs are exciting and depressing; whatever else they may be, time-intervals are straining and relaxing. The earlier study was planned with regard to this assertion of Wundt's.[2] The object of the present study is somewhat different; it is, to make a tridimensional test of the tridimensional theory. Nevertheless, we have, after consideration, selected the same materials to work with,—harmonical clangs and metronome beats. For one thing, it is important to repeat Titchener's experiments, and to confirm or to refute his conclusions; and repetition of the two-dimensional method is, of course, included in the application of a tridimensional method. There was here, also, the added advantage that the technique of these experiments had already been worked out in the Cornell laboratory. For another thing, the stimuli are sufficiently complex to justify the assumption of a complex affective reaction to them, if such a complex reaction occurs at all. Wundt's latest statement of his theory would seem to admit the application of the method to color;[1] but we had nothing to gain by having recourse to color, and a good deal to lose, if time forbade us to employ also the clangs and intervals of the earlier investigation. Lastly, there seems to be no possible objection, from Wundt's own point of view, to our selection of stimuli. "Feelings of strain and relaxation," he says, "are always connected with the processes of attention. Thus, when we expect a sense impression, we note a feeling of strain, and upon the arrival of the expected event, we note a feeling of relaxation. Both the expectation and satisfaction may be accompanied at the same time by a feeling of excitement or, under special conditions, by pleasant or unpleasant feelings."[2] This passage appears, in the light of the previous study, to cover the use of clangs for the production of feelings of all three dimensions. "Again, the series of pleasurable and unpleasurable feelings is united with that of feelings of strain and relaxation, in the case of the affective tones of rhythms. The regular succession of strain and relaxation in these cases is attended by pleasantness, the disturbance of this regularity, by the opposite feeling, as when we are disappointed or surprised. Then, too, under certain circumstances the feeling of rhythm may be of either an exciting or a subduing character."[3] This passage appears to justify the recourse to metronome intervals.

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[1] Moskiewicz (*op. cit.*, 315) writes as follows. "Es hindert nichts, anzunehmen, dass trotz der Einfachheit der Reize die in uns ausgelösten Gefühle komplizierter sind, dass wir auf sie sowohl mit Lust als auch mit Erregung resp. Lösung reagieren. Es ist durchaus nicht ausgeschlossen, dass es andere Reize gibt, auf die wir nur mit Erregung oder Spannung zu reagieren imstande sind. Auch die Gleichheit der Kurven braucht nicht zu verwundern. Warum soll eine Reihe von Reizen nicht in gleicher Weise Lust und Erregung steigernd auf ein und dieselbe Person einwirken können, ohne dass Lust und Erregung identisch wären?" To the first sentence it may be replied that the stimuli (clangs and metronome rhythms) were by no means simple, and that it was partly for this reason that they were selected as a means by which to test the very complexity of affective reaction which the writer assumes. To the second, it may be replied that experiments, however limited, are better than vague conjecture. After working with clangs and time-intervals, Titchener made supplementary experiments with colors (*op. cit.*, 403). What, now, may the "andere Reize" be? Certainly not tastes and smells, which are notoriously pleasant and unpleasant. What then? The suggestion, thus thrown out irresponsibly, is worth nothing, in default of definite reference. To the third and fourth sentences answer has been made in the text. The writer is attacking the objective side of Titchener's argument, and has ignored the subjective side.

[2] Moskiewicz declares: "es wurden nur solche Reize gewählt, die zu zwei Gefühlsdimensionen gehören" (*op. cit.*, 314). There is, we believe, nothing in Titchener's paper that could suggest this idea.

Since, then, there is no positive objection to the use of clangs and metronome beats; and since presumptive evidence in favor of such a procedure is furnished both by Wundt's general view of the affective life and by his criticism of Titchener's experiments; we take the affective efficiency of these stimuli for granted, and ask our observers to pass upon each of them judgments in terms of all three feeling dimensions. The results of experiment are as follows.

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[1] Hollands: *op. cit.*, 219.
[2] Outlines, 1902, 92.
[3] Outlines, 1902, 93; *cf.* Phys. Psych., II, 1902, 286.

I. HARMONICAL EXPERIMENTS.

The method employed in these experiments was the same as that described in Titchener's article. The same harmonical and noiseless pendulum, marking seconds, were used. The 24 tones in the three octaves C-c (64-128 vs.), c^1-c^2 (256-512 vs.), c^3-c^4 (1,024-2,048 vs.) were combined in all possible pairs, thus making a series of 276 pairs of tones. The series was formed by chance, and then so rearranged that the same tone should never occur in two successive pairs. This series was given 12 times to each observer: 6 times upward (↑), *i. e.*, with the lower tone first, and 6 times downward (↓), *i. e.*, with the upper tone first; making a total of 3,312 experiments for each observer, exclusive of 'make-up' experiments.

In each series the observer was asked to report upon one affective quality only.[1]

These twelve series were given in irregular order, but not in precisely the same order to all three observers. In part of the experiments, all three observed together, sitting with their backs to the harmonical, about 2 meters from it, with screens between them. As the observers and the experimenter were engaged at different hours upon other university work, it was impossible to make the experiments at the same hour on succeeding days; but the experiments upon each particular series were made within as few days as possible, and often at the same hour on succeeding days. Two vacations occurred during the progress of these experiments, which further interrupted their regularity.[2] When it was possible to have only two observers at a time, the series was repeated later for the absent observer. For these reasons the 12 series could not be given to all the observers in exactly the same order. The experiments with the harmonical were made during the months of November, 1905—February, 1906, and April, May, 1906.

The observers were given slips of paper upon which to record their judgments, and were informed that they were to make series of judgments upon the relative pleasantness, etc., of the tones in the pairs given. They were instructed to listen to each tone separately, and to make their judgments without bias as to which tone 'ought' to be the more pleasant or unpleasant. They were distinctly cautioned against forming any theory which might influence their judgments, as, *e. g.*, that 'all high tones are liable to be unpleasant.' After 30 to 40 pairs of tones had been given, the observers were allowed a rest of from 3 to 5 minutes, which was generally spent in ordinary conversation. They were encouraged to record introspections during this period, or at the end of the experimental hour, but were cautioned against comparing notes, or discussing results with one another.

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[1] Titchener: *op. cit.*, 388.

[2] Titchener's experiments were made in the short summer session, when the observers could work every day. And, as is usual in the summer sessions, his observers were specializing in psychology, and had not the pressure of other work upon them. Under such conditions it is natural, as will appear to be the fact, that his curves should be somewhat more regular than those obtained in the present investigation.

The actual conduct of an experiment is as follows. The experimenter stands before the organ with one foot upon the raised pedal, the paper containing the series of tones in his left hand, the pendulum bob in his right. At the signal "ready," he releases the bob and presses down the first note to be sounded. After two full swings of the bob (2 seconds), he treads once quickly and once slowly upon the pedal, and keeps a strong, even tone sounding for two swings of the bob. Then, after two seconds of silence, the second tone is sounded for two seconds in the same way. An interval of from 2 to 6 seconds is allowed, between the pairs of tones, for recording the judgment. After the first two days of experimenting, an interval of from 4 to 6 seconds was found to be most comfortable for observers M. and G. Observer W. found this interval very fatiguing, and when she observed alone the interval between pairs was cut down to 2 seconds. The judgments on all the affective qualities required practically the same length of time.[1]

The experimenter exercised the greatest possible care to keep the tones strong and even in intensity, and to prevent noises from the mechanism of the organ. But in spite of care and practice, the pedal occasionally creaked in certain kinds of weather. In such cases, and also when other noises within or without the building disturbed the observers, a record was made of the judgments formed under these disturbed conditions, and the pairs of tones were repeated in make-up series. Practice in the experiment, however, soon developed in the observers such power of concentration that they heard no sounds but the tones, and occasionally surprised the experimenter by insisting that they had not heard noises which he had supposed must make it impossible for them to form a judgment upon the tones themselves.[1]

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[1] None of the judgments exceeded the above time limits, although, as will be shown later, the judgments of the various affective dimensions presented varying degrees of difficulty. It would, of course, have been fairly easy to take approximate measurements of the actual times required for the formation of the judgments; but in view of the tentative and exploratory character of the investigation, this procedure did not appear either necessary or advisable, especially since it would have involved a complication of method both for the observers and for the experimenter.

Certain tones in the scale were at times of different intensity from the rest; *c* was apt to sound suddenly and at high intensity; b^3 and c^4 were rather faint; c^3 occasionally developed a tremolo. The pairs in which c^3 sounded in this way were noted, and repeated in the make-up series. Whenever the observers thought b^3 or c^4 too weak, the pairs in which they occurred were repeated later. Therefore, these variations in the tones themselves can have had no appreciable influence in making the curves irregular, and may be considered negligible. The curves do, however, show quite distinctly the effect of the suddenness and intensity of the tone *c*, the curves being clearly deflected from their general course at this point.

Make-up series were arranged and given to the observers separately. These series consisted of (1) those pairs of tones in which the observer was unable to detect in either tone the affective quality looked for (negative cases); (2) pairs in which the observer found the quality in an equal degree in both tones, and was therefore unable to choose between them (undecided cases); (3) pairs in which the observer decided with difficulty, and even after decision was not sure that the judgment was correct (doubtful cases); and (4) pairs in the observation of which the observer was disturbed.

Observer M. is Miss E. Murray, graduate scholar in psychology. Before these experiments, M. had had 2 years' experience in the psychological laboratory. She was familiar with Wundt's tridimensional theory of the affective qualities, but had not read Titchener's article, and had only a vague idea of the purpose of these experiments. M. is musical.

The experiments were made in the following order: $P\uparrow$, $P\downarrow$, $E\uparrow$, $U\uparrow$, $D\uparrow$, $U\downarrow$, $E\downarrow$, $S\uparrow$, $R\downarrow$, $S\downarrow$, $R\uparrow$, $D\uparrow$. $P\uparrow$ was repeated at the completion of the 12 series. In all, with make-up series, M. made 3,885 comparisons.

(1) *Pleasantness.*[2] After the first series ($P\uparrow$), M. had no

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[1] The absence of a noiseless room in the Cornell Laboratory is justified by experiences of this kind, which show that with practice and keen attention to the problem in hand observers readily become so absorbed in their work that they are oblivious to everything except the particular stimuli upon which they are expected to react. See Titchener: Mind, N. S., VII, 1898, 311; Wirth: Zeits. f. Psychol., XXV, 1901, 129.

[2] Attention may here be called to a distinct difference between the *P-U* introspections, for this and for the other observers, and the introspections with *E-D* and *S-R*. Throughout the investigation, it was the rule that the *P-U* judgments were given, so to say, a smatters of ultimate fact, without possibility of further analysis: the analytical introspections, bearing, *e. g.*, upon the organic reaction set up

M. reported that the tones of the highest octave (c^3–c^4) were harsh and straining, and these were the tones oftenest judged unpleasant. During the first series (*P*↑) she said that they were "as unpleasant as quinine," and made her feel "all screwed up;" that it was very hard to keep her attention on the harsh high tones, and that they sometimes pained her ears; that they did not seem to spread out and give a general reverberation, but remained localized in the ears. In this series M. reported that the high tones gave her "a feeling of revulsion."[1]

(2) *Unpleasantness.* This series was made during the second month of experimenting. No distractions or associations were reported, nor did M. say that any of her judgments were influenced by the musical interval formed by the two tones.

In 27 pairs, both tones were reported pleasant, only one of these pairs being in the highest octave. M. reported that the high tones were very unpleasant. In 7 cases both tones were reported equally unpleasant.

M. described the unpleasantness as "usually a matter of discomfort in the head."

During the series *U* ↓, M. reported that none except the high tones were in themselves unpleasant. The others could be judged unpleasant only by comparing them with pleasanter tones. On the very rare occasions when there was a sensible difference of intensity, the louder tone seemed the more unpleasant. In 10 cases both tones were reported pleasant, only 2 of these pairs being in the highest octave; in 28 other cases the tones seemed equally unpleasant.

P ↓ was a repetition of the first series. M. said that it was difficult to disregard the influence of the musical interval. She was quite unconscious of distractions; for instance, she had not even noticed a steam whistle blown outside the building until the experimenter called her attention to it by asking her if it had disturbed her in making her judgments. No negative cases were reported and only one doubtful case, though M. once said that the decision was often difficult where the two tones were very near together in pitch, and that she had at last, after making a choice, felt that she might perhaps as well have chosen the other tone as the more pleasant.[1]

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[1] The word "feeling" here and in similar instances hereafter is not intended by the observers to express any definite affective quality, but represents a loose use of the word common in conversational English, as well as in the earlier English psychology, in which it covers any mental state or process which is difficult of analysis. "It is plain," says Ward (Encyclopædia Britannica, XX, 40), "that further definition is requisite for a word that may mean (*a*) a touch, as feeling of roughness; (*b*) an organic sensation, as feeling of hunger; (*c*) an emotion, as feeling of anger; (*d*) feeling proper, as pleasure or pain." Common usage goes much farther. We can easily *feel* the force of M.'s thought when, as below, she speaks of an "interval feeling," and later of "a sort of question and answer feeling." The present tendency among psychologists generally is to analyze these complexes, and to limit the word 'feeling' to their affective quality. Curiously enough, however, there seems to be an opposite tendency among certain recent German writers, who are introducing the use of *Bewusstseinslage* in much the same sense as our colloquial 'feeling,' thus avoiding a careful analysis of the complex experienced, and hindering the development of an exact terminology. See, *e. g.*, Mayer and Orth, Zeitschrift f. Psychol., XXVI, 1901, 6.

(3) *Excitement.* M. had no particular difficulty in deciding which of two tones was the more exciting, though these judgments were not quite so easy as those upon *P* and *U.*

In the first series ($E\uparrow$), M. defined excitement as a "feeling of muscular pull." In the series $E\downarrow$ M. reported that the standard of judgment tended to fluctuate; sometimes excitement meant "the amount of disturbance in consciousness," sometimes "the amount of muscular response the sound seemed to demand." Sometimes the greater intensity of a tone made it more exciting;[2] sometimes the judgment, in spite of instructions to the contrary, was based on the relation between tones; after some tones, certain other tones seemed restful and quite lacking in excitement. M. said that sometimes she seemed to get tired of the high tones, and then none of them were exciting. She reported definitely that the feeling of *E* was unpleasant. No distractions or associations were reported. In series $E\uparrow$ there were 7 negative and 28 undecided cases. Of the negative cases, 6 were pairs of tones within the middle octave, which, as the curves show, was the least exciting part of the whole series. In series $E\downarrow$ there were two negative, 19 undecided and 3 doubtful cases.

(4) *Depression.* M. had great difficulty in giving these judgments. When the series $D\uparrow$ was begun, M. reported that all the tones came as either exciting or soothing, and it was possible to get the feeling of depression only by a special effort, —by associating the idea of groaning or shuddering to the low tones. By this reading-in process, 14 low tones were judged depressing; the remaining 111 judgments made on the same day were upon the soothing quality of the tone. On the second day M. tried again to give judgments of depression (sad, melancholy). She reported: "I did not get this feeling sponta-

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[1] It may be worth while to remark again that reflective judgments were not sought in these experiments. What was required and registered was the immediate reaction to the stimulus. *Cf.* the instructions to the observers given above. Apparently, the reflective tendency was unusual with M., for she reported only 11 doubtful cases in the whole series of 3,312 experiments.

[2] Note the sudden jump upward of M.'s *E*-curves at *c.*

neously, but only by trying to see how depressed I could feel while the tone was sounding. I involuntarily adopted the method of breathing out on the tone and delaying inspiration and relaxing other muscles." In this manner the two series of judgments on depression ↑ and ↓ were made. In series *D* ↑ there were 19 negative and 14 undecided cases; in series *D* ↓, 7 negative and one undecided case. At the end of the series *D* ↓, M. reported: "the ability to annex a depressing feeling to tones seems to vary," and "it seems easier to be depressed on a high note after a number of high notes have been given."

In order to obtain comparable judgments upon the same quality on successive days, the experimenter showed M. her first introspections on depression, each time that judgments on depression were sought.[1]

In response to a question as to the relation of *D* to *E*, M. wrote: "I should hardly say that depression is the opposite of excitement; that is, in this series, those notes that were not depressing were not thought of as exciting or neutral but as irritating, disquieting or disturbing."

(5) *Strain.* In the first days of these experiments, M. said that the feeling of strain seemed to be merely the effort to attend to a tone, that the strain was usually unpleasant, and that when she was in doubt as to which tone was the more straining she was inclined to rely upon the unpleasantness of it. Therefore we are not surprised to find the strain curves similar to those of unpleasantness. At the end of the experiments on strain, M. reported as follows: "Straining is not a simple immediate judgment, as is the case with judgments upon *P* or *U*. I should not be able to make the judgment unless I was prepared in a special way to respond to the stimuli. I do not get any judgment on strain unless I follow the notes somehow, with inspiration or expiration, or with general muscular contraction; except in the case of high squeaking tones which give a sense of involuntary strain in the ear and head." In series *S* ↑ there were 12 negative and 8 undecided pairs; in series *S* ↓ there were 11 negative and 19 undecided cases.

(6) *Relaxation.* M. had great difficulty, again, in obtaining a feeling of relaxation. A trial series was given of 5 pairs

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[1] The same plan was adopted in the experiments on strain and relaxation. These responses tended to be so artificial that there was danger that M. would forget how she obtained the judgments, and by adopting some new method for their production give a different series of judgments each day. By refreshing her memory, the whole curve for each quality was made up of responses of the same kind.—It need hardly be said that the associations involved in these judgments are of an entirely different nature from the casual and unsystematic associations referred to above as quickly disappearing in the course of affective work.

of tones ↑, and M. said that she had been judging them with regard to their relaxing quality. But, upon further consideration, she decided that she had been confusing relaxation with depression. A trial series of 5 tones ↓ was then given, and as both M. and G., who was observing with her, found it much easier to judge of relaxation in the downward series, the series *R* ↓ was given before the series *R* ↑.

On the first day of experimenting on relaxation M. wrote: "The drop, rather than the second tone itself, seems to be relaxing; usually there is no relaxation with the first note, unless it is very high; the negative cases seem to occur when there is no interval feeling." During the next series she wrote: "Apparently the relaxing feeling is merely the release of attention from the first note. Each pair of notes gives you a sort of question and answer feeling." The following day M. gave judgments on strain, and at the end of the hour wrote: "In making judgments on strain I realize that, in the previous judgments on relaxation, I was not judging of the affective quality of the individual notes at all—which very seldom gave anything like a feeling of relaxation—but merely on the transition from one to the other. If you do not attend to the transition or interval but to each note separately, you get either the 'straining' feeling or absence of any feeling,—not relaxation."

During the series *R* ↑, M. reported: "Relaxation seems to be similar to depression. I get the judgment 'relaxing' by trying to see how relaxed I can be while the note is sounding. Relaxation seems to be associated with a visual picture of height. It seems to require a greater effort to feel relaxed on high notes."

After the completion of both series of experiments on relaxation, M. said that the feeling of relaxation was like the muscular strain of stooping downward; that she arranged the tones in a visual line sloping downward from right to left, and that when a high was followed by a low tone she felt as if she were bending downward with the second tone; in other cases the feeling arose when tones were lower than she had expected them to be; when there was no bending with the tone, there was no feeling of relaxation.

In *R* ↑ there were 12 undecided and two negative cases.

In *R* ↓ there were 3 undecided and 24 negative cases.

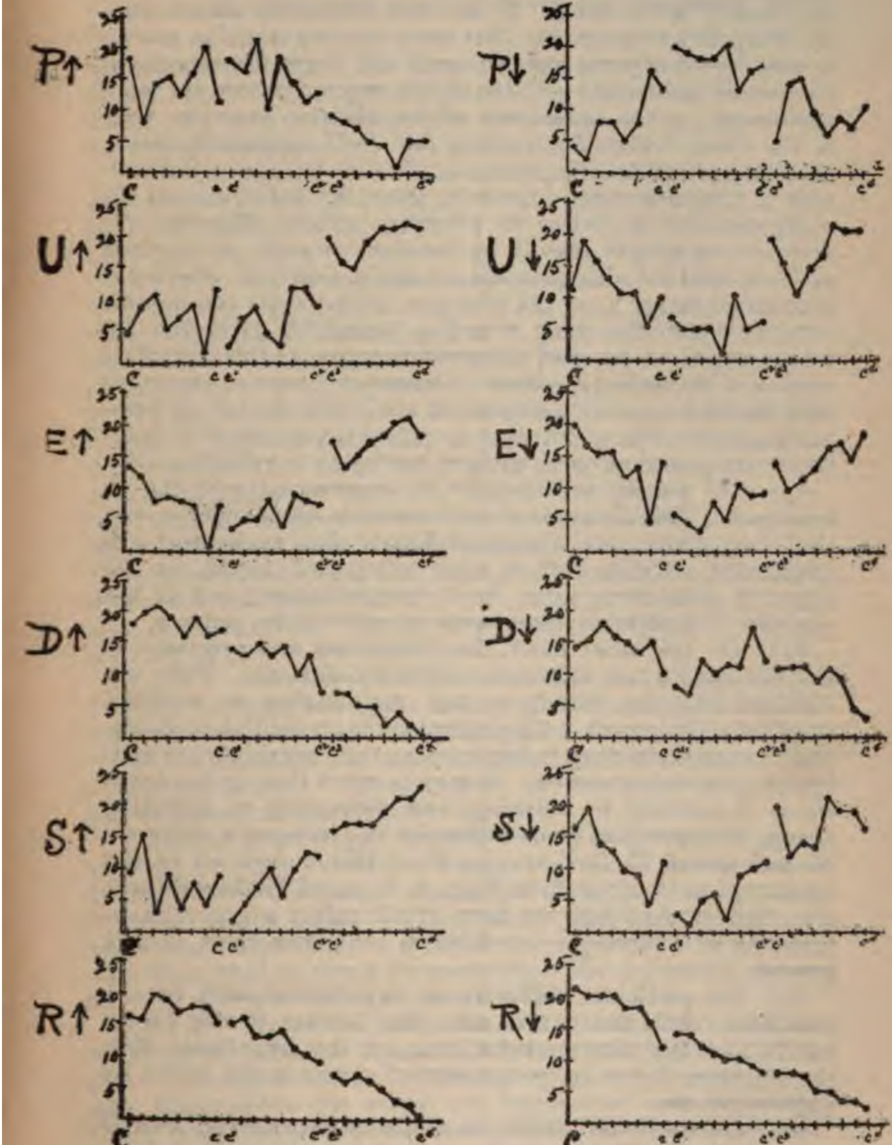
SUMMARY OF RESULTS FOR M.

Putting together, now, the subjective and objective evidence to be derived from introspections and curves, we arrive at the following conclusions.

(1) The *P* and *U* curves take typically opposite courses.

(2) The judgemnts of *P* and *U* are direct or immediate

PLATE I.



judgments. This does not mean, of course, that the affective quality *always* attaches blindly, so to say, and with introspective finality to the clang. It has been sufficiently shown, *e. g.*, by Whipple's experiments, that every sensory stimulus sets up a wide-spread organic reaction, over and above the exercise of its specific influence.[1] At times, the organic factors are more prominent, as the sense-basis of the affective reaction, than is the clang itself. In terming the *P–U* judgments direct, therefore, we mean only that the affective quality of pleasantness or unpleasantness appears in consciousness at once as the clang-stimulus produces its conscious effect. Whether the attention is caught mainly by the clang as such (which then appears 'intrinsically' pleasant or unpleasant), or whether it is centered rather upon the muscular adjustments and organic 'sets' to which the clang stimulus necessarily gives rise (in which case these sets and adjustments appear as the immediate vehicle of the feeling),—these alternatives, however important their decision in other connections, are irrelevant for our present purpose. The point here to notice is that the *P–U* judgments are passed at once, without hesitation or reflection.

(3) No particular difficulty is experienced with the *E*-reactions. The *E*-curves closely resemble the *U*-curves, and the introspective record states definitely that excitement was unpleasant. Excitement is thus interpreted by M. as the opposite of soothing calm, quiet tranquilization; not as the opposite of depression in the sense of melancholy, sadness.

(4) On the other hand, the *D*-reactions are, precisely as in Titchener's experiments, relatively difficult. They are obtained only by setting-up and maintaining an artificial standard of judgment. The curves show an oscillatory downward course from deep to high tones; the judgments are matters of emotive association. It may be noted that, in her opposition of exciting to soothing, and depressing to irritating clangs, M. shows the same tendencies as Titchener's observers N. and second G. (*op. cit.*, 394 ff.). Her curves are of the same type as G.'s curves in Figg. 8, 11, 12 of Titchener's article. So far, therefore, we have at all points a general confirmation of Titchener's conclusions. We now come to new ground.

(5) No particular difficulty is experienced with the *S*-reactions. But the curves are very similar to the curves for *U*, and the introspections bear out the hypothesis that the affective factor in judgments of strain is the factor of unpleasantness.

(6) On the other hand, the difficulty experienced with *D* recurs with *R*. The mechanism of judgment in the two cases

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[1] *American Journal of Psychology*, XII, 1900-1, 444.

is much the same; and the R-curves, like the D-curves, show an oscillatory downward course from deep to high tones. There can be little doubt that the curves are associative artifacts.

Observer G. is Mr. L. R. Geissler, assistant in the psychological laboratory. G. had had 2 years' experience in experimental work. He had no more definite knowledge of the purpose of the experiments than M. He also is musical.

The experiments were made first in the following order: $P\uparrow$, $P\downarrow$, $D\uparrow$, $U\downarrow$, $E\downarrow$, $S\uparrow$, $R\uparrow$, $U\uparrow$, $S\downarrow$, $E\uparrow$, $R\downarrow$, $D\downarrow$; and then repeated in the order $P\uparrow$, $E\downarrow$, $S\uparrow$, $D\uparrow$, $U\downarrow$, $R\downarrow$, $E\uparrow$, $S\downarrow$, $P\downarrow$, $D\downarrow$, $U\uparrow$, $R\uparrow$: in all, 6,845 comparisons.

(1), (2). *Pleasantness* and *unpleasantness.* Although G. reported only one negative case ($P\uparrow$) in the 1,104 experiments upon P and U, he seemed at first to have difficulty in deciding which of two tones was the more pleasant or unpleasant. In the series $P\uparrow$ he reported 63 undecided cases, in $P\downarrow$, 58, in $U\uparrow$ 64, and in $U\downarrow$ 21. He said that he found it difficult to choose between tones very far apart or very near together in the scale. But when these pairs were repeated later, the total of all undecided cases in P and U was reduced to 2. He reported the influence of interval on only one day. On that day he said that the succeeding tones often suggested a melody. This he found somewhat distracting, and it made the judgment of pleasantness more difficult. G. reported no other distractions or associations. He had a particular dislike for *c*, which is clearly shown in all his curves.

(3), (4). *Excitement* and *depression.* G. had a good deal of difficulty in experiencing the feeling of excitement, and finally concluded that "if there is anything more than unpleasantness at all" it must be the state of muscular restlessness which some tones aroused. We note that the curves of excitement and unpleasantness are almost identical.

"Depression is a vague, general feeling of quiet, letting-go, avoiding activity; one feels so lazy that it seems an unpleasant interruption to the general state of mind to have to record a judgment on the tones." This state of mind, he says, is pleasant, though depression is a rather more passive and neutral state than what he terms pleasantness. The depression curves are very similar to those of pleasantness.

(5), (6). *Strain* and *relaxation.* Strain and relaxation are described in muscular terms. G. has a tendency to sing the tones, and those that are within his range are easy and pleasant; those above his range are unpleasant and straining. These muscular strains are localized in the throat, face and forehead. Some, especially the very high tones, are so unpleasant that it is a constant strain to listen to them. Then

G. finds strain sensation in the ears, and various muscular contractions involved in the effort of attention. The curves bear out the close connection between strain and unpleasantness which is indicated by these introspections.

Relaxation seems to be merely the absence of the various kinds of muscular strain just enumerated. G. had great difficulty with these judgments, because he "had no standard. There was nothing but the general diffused feeling of relaxed muscles and a vague, undisturbed state of mind." This experience was generally pleasant. We note that the curves of relaxation are good opposites of those of strain, and run practically the same course as those of pleasantness.

Summary of Results for G.

(1) The P–U curves show typically opposite courses.

(2) After initial practice, the P–U judgments are direct or immediate.

(3) Excitement is with difficulty differentiated from unpleasantness, and what difference there is appears simply as a difference in the range of the organic vehicle of the feeling. The curves of E resemble the curves of U.

(4) Depression, the given opposite of (unpleasant) excitement, is interpreted to mean soothing restfulness, undisturbed calm. It is pronounced pleasant, and the curves agree with the curves of P. The curves are of the same type as those of Titchener's first observer G. (*op. cit.*, Figg. 1-6).

(5) S and R are described in muscular terms, S being unpleasant and R pleasant. The curves agree in general with the U–P curves.

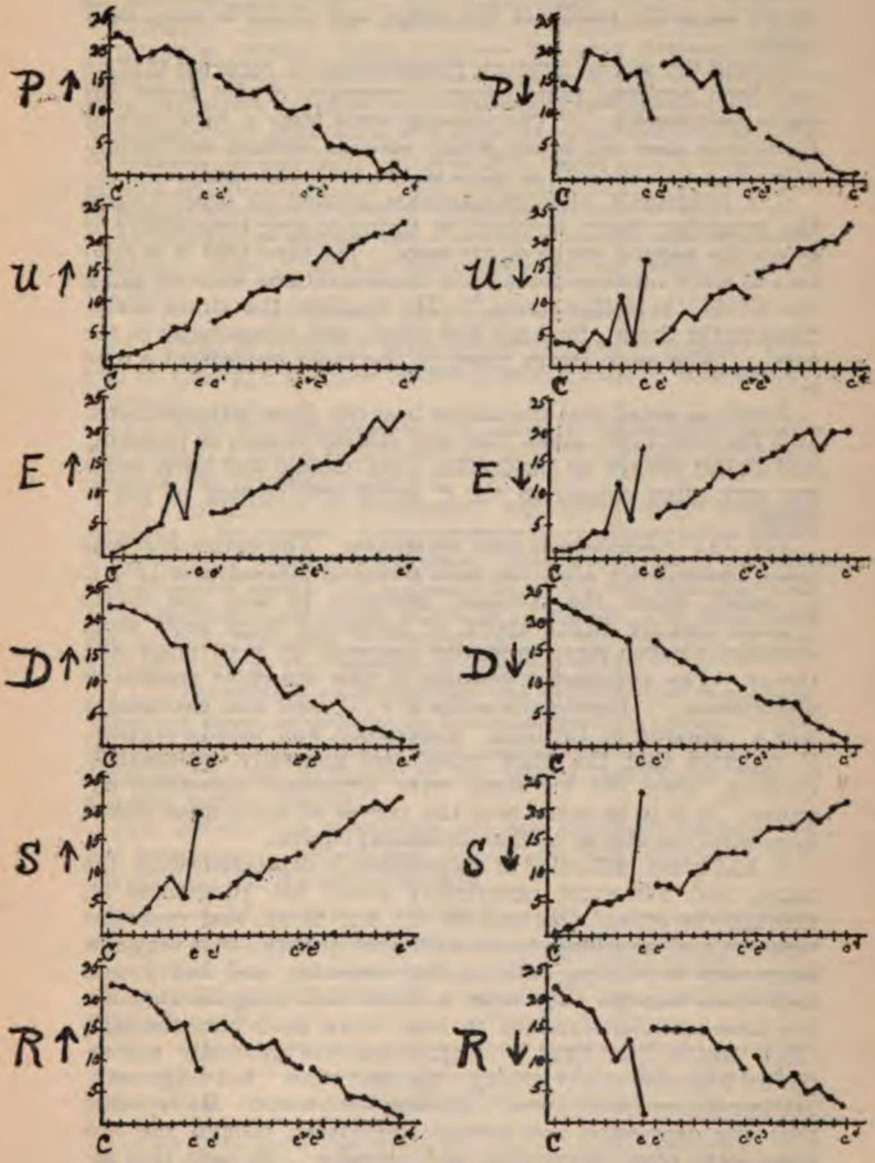
Observer C. is Mr. J. H. Coffin, assistant in the psychological laboratory. C. had had 2 years' experience in experimental work before these experiments were begun. He was familiar with Wundt's tridimensional theory, but had not read Titchener's article, and had only a general idea of the purpose of the experiments. He is musical.

The experiments were made in the following order: $P\uparrow$, $E\downarrow$, $S\uparrow$, $D\uparrow$, $U\downarrow$, $R\downarrow$, $E\uparrow$, $S\downarrow$, $P\downarrow$, $D\downarrow$, $U\uparrow$, $R\uparrow$;—3,312 comparisons.

In all the experiments with the harmonical, C. reported a constant tendency to sing the tones given. His judgments were largely dependent upon the ease or difficulty of the muscular adjustment to tones in different parts of the scale.

(1), (2). *Pleasantness* and *unpleasantness.* During the first series given ($P\uparrow$), C. reported that he was influenced in his judgments by the intensity, quality, and clang-tint of the tones, and occasionally by the intervals they formed. This last he thought was due to the fact that he had a constant

PLATE II.



tendency to sing the tones. Those tones were the pleasantest which lay in the middle of his range, and could be sung most easily.

During the second series on pleasantness, C. reported that he had been noticing some organic sensations which stood out quite prominently. "The pleasant tones start a little jerk or thrill in or near my heart, which spreads upward and culminates in a sort of thrill or glow in the neck and cheeks."

C.'s judgments of unpleasantness seemed to depend upon the muscular strain involved in trying to sing tones above or below the natural range of his voice. He says that it is difficult to write introspections upon unpleasantness without using the words "muscular strain." He localizes the strain sensations in the throat, forehead and chest, and occasionally in the ears. "The more strain there is, the more unpleasant a tone is."

It will be noted that the curves bear out these introspections. It is the very high tones that are seldom chosen as pleasant, and almost always as unpleasant; the middle and lower tones are very often chosen in the *P* series and seldom in the *U* series.

(3), (4). *Excitement* and *depression.* The series *E*↓ was given immediately after the first series on pleasantness (*P*↑). C. noted again the constant tendency to sing the tones. "Tones that are either above or below my range seem more stirring—awaken more muscular response in both chest and throat. The excitement consists in this muscular tension or disturbance." During the series *E*↑, C. said that excitement was a "stirring or thrilling" experience, and mostly organic. C. reported that the high tones were generally unpleasantly exciting, while the low tones were sometimes pleasantly exciting. It is to be noted that the curves of excitement follow in general the course of the unpleasant curves.

C. had great difficulty in experiencing depression with the tones, and felt great uncertainty about his judgments all through the series. He said he did not think that muscular tensions had anything to do with this quality; that very low tones were depressing because they were big and heavy, and high ones because they were so little and insignificant; the low ones sometimes seemed to bear down upon him, the high ones "made him tired." Depression was generally accompanied with difficult breathing; his chest often "felt weighted." He sometimes found himself holding his breath. He reported that the depression was usually unpleasant, though the low tones were often depressing and pleasant. He said that depression was not in any sense the opposite of excitement. We note that the curves of depression are quite similar to those of unpleasantness, differing mainly in the fact that the tones of the lowest octave were the most depressing.

(5), (6). *Strain* and *relaxation*. "Strain is clearly a case of muscular tension; those tones are the most straining which require the most muscular effort to sing them." To this he adds the strain of muscular adjustment in the ears, which in the case of very high tones was quite as noticeable as the strain occasioned by the effort to sing the tones. This strain was always unpleasant. We note that the curves of strain are strikingly similar to those of unpleasantness.

Relaxation is simply the absence of strain, and is always pleasant. The *R* curves are practically the same as the *P* curves.

SUMMARY OF RESULTS FOR C.

(1) The *P-U* curves take typically opposite courses. They are of the type of those obtained from Titchener's second observer G.

(2) The *P–U* judgments are direct or immediate, though the spread of the organic reaction is more marked than in the cases of the previous observers.

(3) Judgments of excitement are based upon muscular tensions. In general, *E* corresponds to *U;* though the lowest tones may be pleasantly exciting. The curves bear out this introspective report. It is clear that C. did not place any definite affective value upon the term 'exciting,' but took it (as the muscular tensions suggested) now as the opposite of melancholy, now as the opposite of soothing calm.

(4) Judgments of depression are difficult and uncertain. They are based, according to the introspective report, in part upon associations, in part upon unpleasantness. The curves show the former influence in the lowest octave, the latter in the two higher octaves.

(5) Strain and relaxation are also muscular matters, and are pronounced respectively unpleasant and pleasant. The curves bear out the introspections.

Observer W. is Miss M. C. West, graduate scholar in philosophy. W. had had no experience in experimental psychology before this year, was unfamiliar with Wundt's tridimensional theory, and did not know the purpose of these experiments. She is musical.[1]

The experiments were first made in the following order: $P\uparrow$, $P\downarrow$, $D\uparrow$, $U\downarrow$, $E\downarrow$, $S\uparrow$, $R\uparrow$, $U\uparrow$, $S\downarrow$, $E\uparrow$, $R\downarrow$, $D\downarrow$. The curves obtained were irregular, and it seemed

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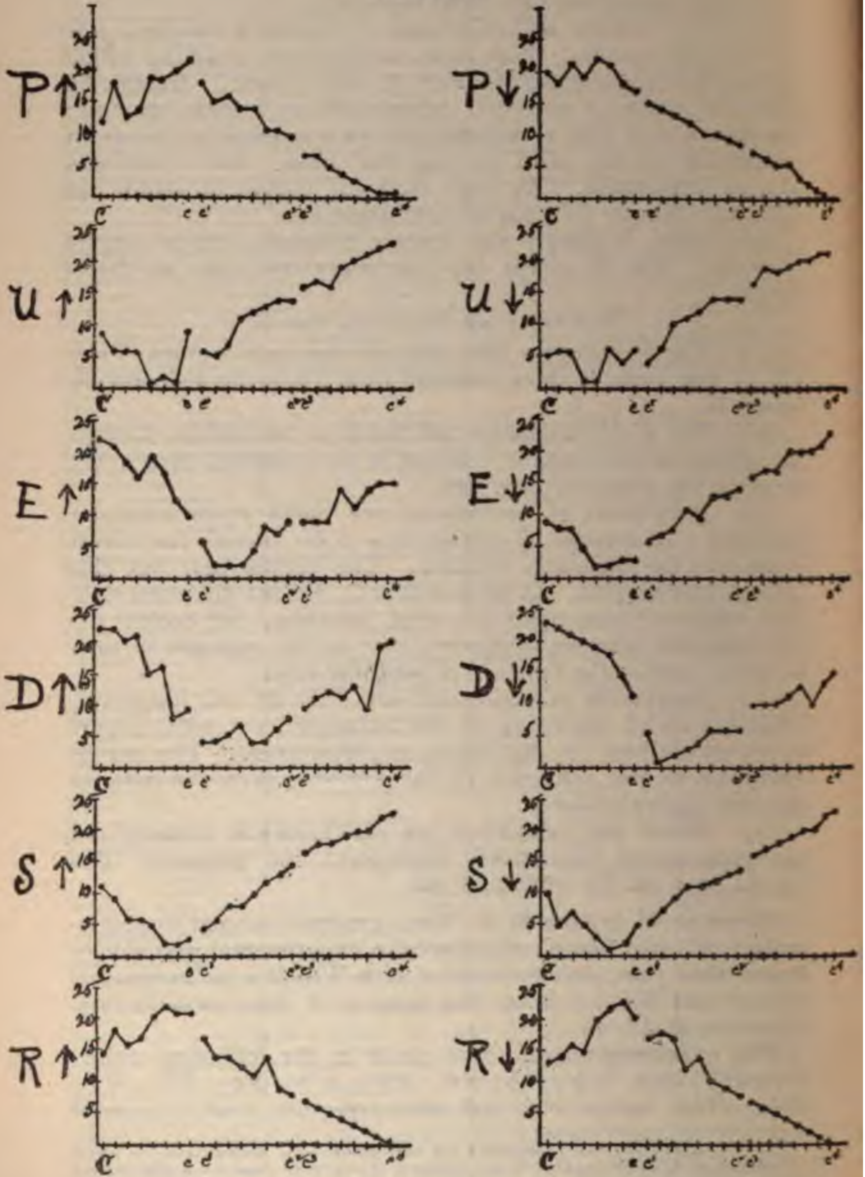
[1] If we classify our observers as objective and subjective in type (Titchener, Experimental Psychology, I, ii, 1901, xxv f.), the order from objective to subjective will be: M., C., G., W.,— and the intervals will become larger as the series progresses.

PLATE III.



probable that W. had not precisely understood her instructions. Accordingly, the whole series of experiments was repeated later in the following order: *P*↑, *E*↓, *S*↑, *D*↑, *U*↓, *R*↓, *E*↑, *S*↓, *P*↓, *D*↓, *U*↑, *R*↑. The curves obtained in the second series were almost as irregular as those of the first series, but showed similar tendencies. The curves published are formed by combining the four curves upon each affective dimension. W. made 6,809 comparisons.

(1), (2). *Pleasantness* and *unpleasantness.* These judgments were easy and immediate. In the *P*-series there were a good many cases in which both tones were reported unpleasant. W. was sometimes influenced by the intensities of the tones, and occasionally by the intervals they formed. Once she wrote that it was difficult "to rid the tones of ideational associations," but did not specify what these were. W. said that unpleasantness arose when the tones were harsh, rough, scraping, nasal, or shrill. She found it difficult to decide which tone was the more unpleasant when the two were widely separated on the scale.

After the first few days of experimenting, W. was not easily distracted. The loud jingling of sleigh bells, and the noise of an electric car groaning up-hill, were not even noticed.

(3), (4). *Excitement* and *depression.* W. reported that the exciting quality of the tones seemed to "wake her up muscularly;" that the excitement was a lively, active, motor feeling, characterized by tightening-up of the muscles and short breathing. Sometimes she noted a tightening of the ear-drums. She said that high and loud tones seemed to be most exciting; also thrilling tones, and those that reminded her of a trumpet or a horn. She found excitement pleasant rather than unpleasant, though she twice reported that unpleasant tones were occasionally exciting.

In order to get judgments upon depression, W. found it necessary to put herself in a melancholy mood; which, on some days, was difficult to do. This mood was characterized by "a constrained, paralyzing feeling," "a weight on the chest," "slow, deep breathing," "a feeling of weariness and heaviness." W. said, during the last *D* series, that there were two alternating elements in the mood of depression; a passive longing which was pleasant, and a feeling of fear or dread that was distinctly unpleasant.

(5), (6). *Strain* and *relaxation.* W. described strain in muscular terms. She reported a general feeling of tense muscles, and a contraction of the ear muscles, both accompanied by short breathing. She said that the high tones were the more straining, though occasionally low tones were straining, when they were especially intense.

Strain seemed to her always unpleasant. In fact she thought that the strain sensations were the main element in the unpleasantness of the tones.

To get the feeling of relaxation, W. found it necessary to use an artificial method similar to those used by M. in the series on *D* and *S*. "I try to take a long breath," she writes, "and, if the tone does not interfere with a good long breath and a general let-go feeling, I call it relaxing; if it makes me draw together, I call it less relaxing." "I can't relax on an unpleasant tone." Relaxation seemed to her a pleasant feeling due to the absence of muscular strains.

SUMMARY OF RESULTS FOR W.

(1) The *P-U* judgments are direct. The curves, though irregular, take generally opposite courses.

(2) The *E*-judgments are based upon muscular tensions. *E* is generally pleasant, sometimes unpleasant. The curve is very irregular; though it seems to resemble the *P*-curve at first, and the *U*-curve in its later course.

(3) The *D*-judgments are artificially mediated, and there seems to have been no constant standard of judgment. W. had first to 'put herself in a melancholy mood,' to pass judgment at all; and the mood, when realized, had alternations of pleasant and unpleasant phases. The curve perhaps resembles the *U*-curve at first, and the *P*-curve in its later course. It is clear, however, that W. attached no definite affective value to the terms *E* and *D*.

(4) *S* is reported unpleasant, and the *S*-curve is a very fair reproduction of the *U*-curve.

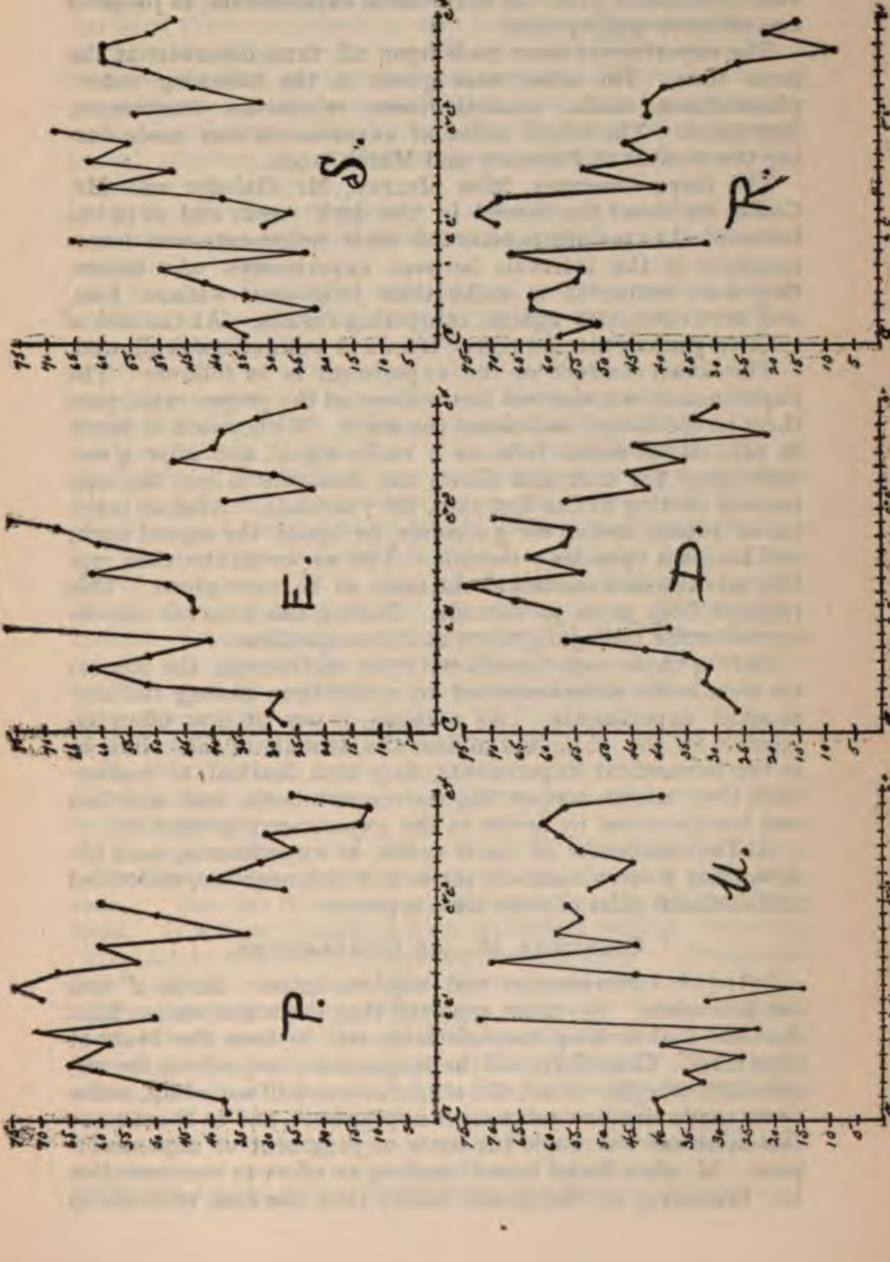
(5) The *R*-judgments are associatively mediated. On the whole, the *R*-curve bears out the introspective verdict that *R* is pleasant.

II. METRONOME EXPERIMENTS.

The method and apparatus used in these experiments were the same as those described in Titchener's article. Two metronomes of equal rates were placed in sound-proof boxes. From each box a rubber tube, fitted with a brass cock, carried the sound to a brass Y, whence the sound passed through a metal tube into the adjoining dark room, where it was distributed by means of a megaphone funnel.

The 14 rates used by Titchener were combined in all possible pairs, making a series of 91 tests. These rates were: 44, 50, 60, 76, 92, 108, 120, 132, 144, 152, 160, 176, 192, and 208 in the 1 min. The series was formed by chance, and then so arranged that the same rate should never occur in two successive pairs. This series of 91 pairs of rates was given 6 times, thus making a total of 546 experiments. In each pair, the

PLATE IV.



slower rate was always given first. In each series the observers were asked, as in the harmonical experiments, to judge of one affective quality only.

The experiments were made upon all three observers at the same time. The series were given in the following order: pleasantness, strain, unpleasantness, relaxation, excitement, depression. The whole series of experiments was made during the months of February and March, 1906.

The three observers, Miss Murray, Mr. Geissler and Mr. Coffin, sat about the funnel in the dark room, and as in the harmonical experiments recorded their judgments and introspections in the intervals between experiments. As before, they were instructed to make their judgments without bias, and were cautioned against comparing results. At the end of each 10 pairs of rates, a rest of from 2 to 5 minutes was allowed.

The actual conduct of the experiment is as follows. The experimenter sets the two metronomes at the proper rates, puts them in the boxes, and closes the doors. With watch in hand, he taps on the metal tube as a ready signal, and after 5 seconds opens the cock and allows the observers to hear the metronome beating at the first rate, for 7 seconds. After an interval of silence lasting for 5 seconds, he opens the second cock, and leaves it open for 7 seconds. The experimenter then rapidly sets the metronomes at the rates to be next given. This requires from 30 to 50 seconds. During this interval the observers write their judgments and introspections.

During these experiments with the metronome, the observers were much more disturbed by noises than during the harmonical experiments. As a result, it was at first often impossible for the observers to hear the stimuli at all. But, as in the harmonical experiments, they soon learned to concentrate their attention upon the metronome beats, and were less and less disturbed by noises as the experimenting went on.

At the conclusion of the 6 series of experiments, each observer was given a make-up series in which negative, undecided and doubtful pairs of rates were repeated.

OBSERVER M. 595 COMPARISONS.

(1), (2). *Pleasantness* and *unpleasantness*. Series *P* was the first made. M. twice reported that the sound was so faint that she had to keep uncomfortably still to hear the beats at slow rates. This effort, and the suspense accompanying the uncertainty whether or not the stimulus was still sounding, sometimes made the slow rates seem unpleasant; and in three cases this faintness was made the basis of judgment of unpleasantness. M. often found herself making an effort to accommodate her breathing to the slower rates; then the ease with which she could make this accommodation determined the pleasantness or unpleasantness of the rates. Rapid rates often made her head throb unpleasantly, and sometimes gave her "an uncomfortable feeling in one ear." A slight difference in pitch between the two metronomes was noted on the second day of experimenting; this was made the basis of judgment in one case. On the same day she said that the rates sometimes suggested pleasant or unpleasant associations which tended to influence her decisions.

In the *P* series there were 5 negative, 5 undecided, and 3 doubtful cases; in the *U* series, 11 undecided, 2 doubtful, and no negative cases.

(3), (4). *Excitement* and *depression*. M. reported that in reacting to the exciting character of the rates, consciousness was completely filled with the processes (mainly muscular) involved in the excitement; that there was a succession of muscular strains "in the effort to keep up with the rate," accompanied by more or less confusion and hurry,—a sort of "driven feeling;"[1] that there was also "a continuous whirr in the head." The higher degrees of this experience were decidedly unpleasant. As to depression, M. writes: "I think I could not give judgments upon this unless I especially directed my attention to it beforehand, and regarded it as the opposite of excitement." In order to obtain the depressed feeling it was often necessary to call up associations. When experienced, depression was accompanied by the relaxation of certain muscles and by slow breathing; M. felt either quite indifferent to the stimulus, or was only slightly affected by it. The state in general was neither positively pleasant nor positively unpleasant, but rather indifferent.

In series *E* there were 2 negative, one doubtful and no undecided cases; in series *D* 6 negative and no undecided or doubtful cases.

(5), (6). *Strain* and *relaxation*. M. described the feeling of strain as a "general muscular tension, due to a rigid attitude." One day this strain seemed to be localized in her forehead. It was accompanied by a breathless feeling. She was "completely absorbed in following the stimulus." Consciousness seemed crowded, so that there was no room for associations. On another day she thought the strain localized in her ears; it accompanied each stroke of the metronome, but disappeared at the least relaxation of the attention. The strain often grew less after the first few beats of a group. She reported that the feeling of strain was generally unpleasant. The second metronome seemed at times to her to be of a slightly higher pitch; this, she thought, increased the straining quality of the beats.

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[1] 'Feeling' in the general sense of *Bewusstseinslage*.

M. describes relaxation as a pleasant feeling, with no confusion or effort. The rates which were accompanied by relaxation were those which could be easily followed with the throat muscles, or did not conflict with an easy rate of breathing. Relaxation seemed to be the opposite of excitement as well as of strain. She said that the "whirr in the head" noted above under excitement was quite lacking.

In the series *S*, there were 1 negative, 9 undecided, and 2 doubtful cases; in the series *R*, 1 undecided, 1 doubtful and no negative cases.

SUMMARY OF RESULTS FOR M.

(1) The *P* and *U* curves take roughly opposite courses. In general type they resemble the curves of Titchener's observers M. and D. (*op. cit.*, 400).

(2) Judgments of *P* and *U* were direct or immediate. The organic factors in the sensory vehicle of the feeling were often prominent.

(3) In the case of *E* and *D*, judgment was passed mainly in terms of sensation ('muscular' sensations, in the widest sense of that phrase). We find, accordingly, that the curves tend to take a straight course, the quickest rates being the most exciting, and the slowest the most depressing. Increase of *E* is unpleasant, and the latter half of the *E*-curve shows a similar course to that of the *U*-curve. The *D*-judgments are difficult; they are regarded merely as the logical opposite of the *E*-judgments, and are often mediated by association. No constancy of affective tone is reported. It must, therefore, probably be concluded that this curve is an associative artifact.

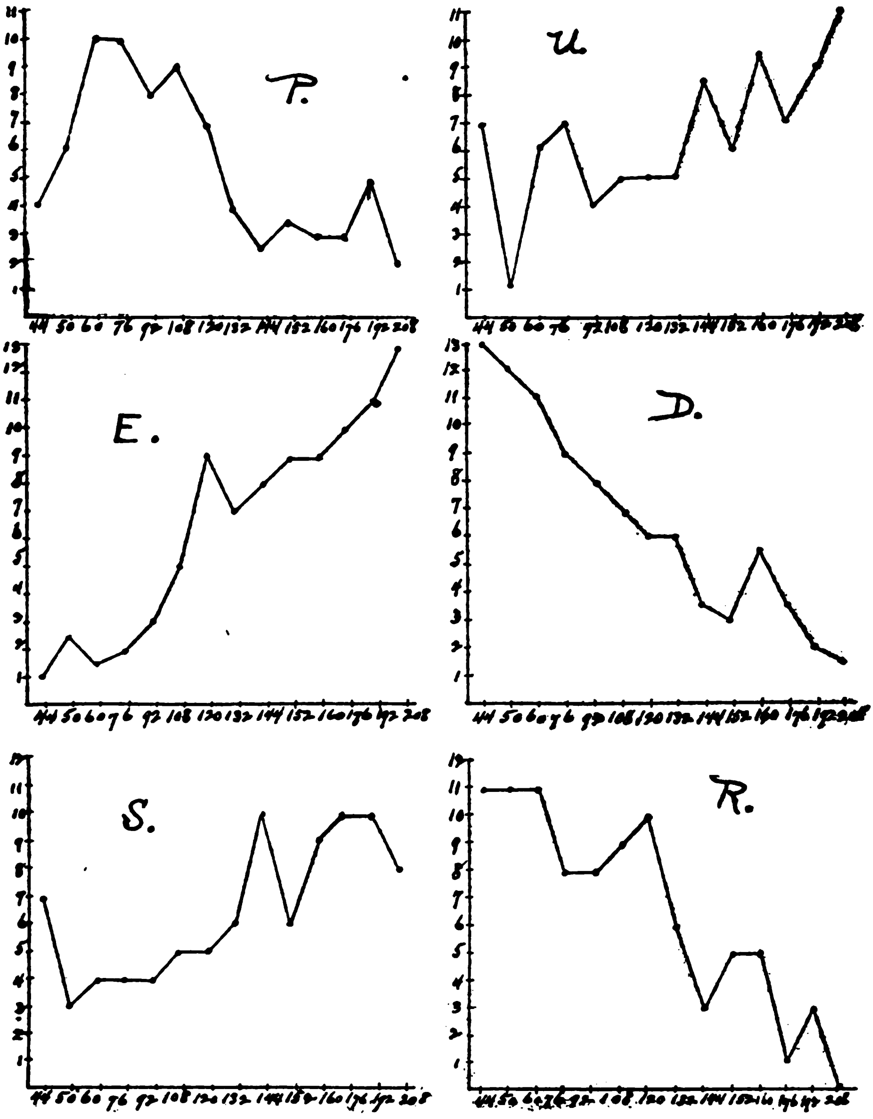
(4) Judgments of strain and relaxation were easy and direct. The introspections resemble those given for *P* and *U*, and the curves (as in Titchener's experiments) accord well with the curves for *P* and *U*.

(5) Neither in the *E-D* nor in the *S-R* experiments is there evidence of any affective factor other than *P-U*. It is noteworthy that M. regards *R* as the opposite both of *S* and of *E*.

OBSERVER G. 606 COMPARISONS.

(1), (2). *Pleasantness* and *unpleasantness*. With the slow rates, G. had a feeling of strained expectation or suspense, which made them seem unpleasant. Very rapid rates gave him an uncomfortable, hurried feeling. "It is," he writes, "always unpleasant for me to be hurried faster than I want to go." In cases where the two rates were nearly equal, he

PLATE V.



found it impossible to decide which was the more pleasant or unpleasant without the help of associations. During the first two days, G. said he could detect no difference in rate between the following pairs: 50 and 60, 60 and 76, 92 and 108, 120 and 132, 144 and 152, 152 and 160, 176 and 192. G. reported a slight occasional difference in pitch between the two metronomes, which sometimes influenced his judgments. During the experiments upon excitement, G. reported that when the slow rates approximated his natural rate of breathing, they seemed decidedly pleasant.

In series *P* there were 7 doubtful, no negative, and no undecided cases; in series *U*, 2 negative, 16 undecided and 2 doubtful cases.

(3), (4). *Excitement* and *depression*. G. seems to have made his judgments upon these two qualities principally by the help of the train of ideas suggested by the rates. The faster rates suggested horses racing, rapid musical compositions, etc.; the slower, bad weather, funeral marches, etc. Without the help of these associations he was unable to feel either excited or depressed. He described the feeling of excitement as muscular restlessness attended by "a restlessness of mind" in which these associations rapidly succeeded one another. The higher degrees of excitement were unpleasant.

In the series *D*, G. chose the first rate of the pair every time, as if he had decided beforehand that slow rates must be the more depressing. No negative, undecided or doubtful cases were reported in either series.

(5), (6). *Strain* and *relaxation*. On the first day of the strain series, G. reported a feeling of strained expectation, which was especially noticeable with the slower rates. This is the same observation as that which he made during the series *U*. Later he said that it was difficult to keep from confusing strain with unpleasantness or with the sensations of muscular tension accompanying the effort of attention. In 4 cases, both rates seemed the same (44 and 50, 50 and 60, 60 and 76, 76 and 92).

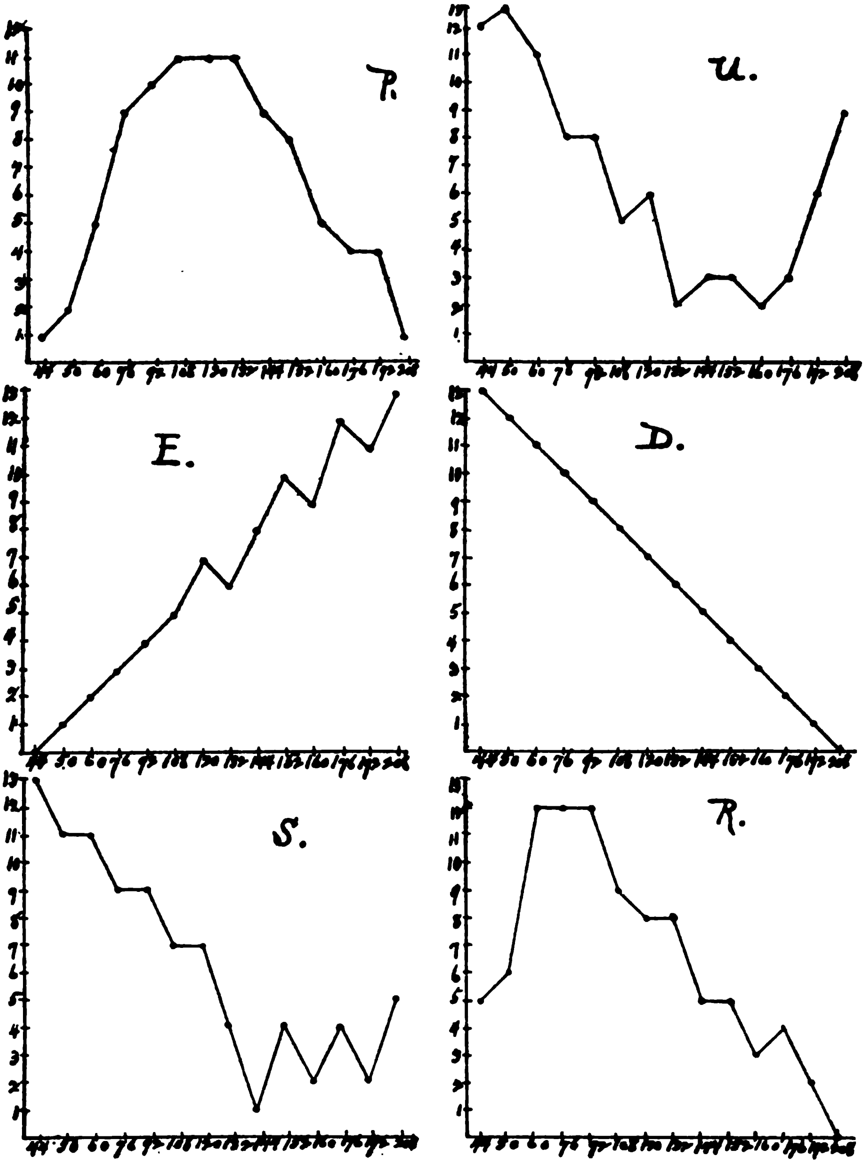
G. described relaxation in organic or muscular terms as restfulness, "settling down," quietude. This was easiest to experience with the rates of moderate speed. Very slow rates, he said, "seemed to hold him in suspense" and so made relaxation impossible.

In series *S*, there were three negative, 14 undecided, and 6 doubtful cases; in series *R*, no negative, 6 undecided and 4 doubtful cases.

SUMMARY OF RESULTS FOR G.

(1) The *P-U* curves take opposite courses, which agree in general type with those of the curves already mentioned.

PLATE VI.



(2) Judgments of *P-U* are direct.

(3) The *E-D* curves wear an artificial appearance, and the judgments are avowedly mediated by associations. There is no evidence of an affective factor, save that high degrees of excitement are pronounced unpleasant. The curves approximate straight lines, the highest rates being the most exciting, the slowest the most depressing.

(4) Judgments of *S* and *R* are based partly upon 'muscular' sensations, partly upon the *P-U* factors. In general the *R*-curve resembles that of *P*, the *S*-curve that of *U*. Indeed, when we consider the degree of practice implied in these experiments, we may probably say that the curves agree as well together as would pairs of *P* or of *U* curves.

In all the experiments with the metronome, C. noted a constant tendency to "keep time" with the beats by some sort of muscular movement, with the throat, feet, hands, etc., and his judgments were based upon the ease or difficulty of this procedure. C. made 556 comparisons.

(1), (2). *Pleasantness* and *unpleasantness*. These judgments were directly determined by the ease or difficulty of the muscular response to the rates. In very rapid rates, C. grouped the beats by fours and then counted one to each group, accenting the first beat. In medium rates, the single beats suggested a rhythm and the muscular response was pleasant. In two cases (pairs 144 and 152, 176 and 192) the rates seemed to be the same. Where they were widely different, he found it difficult to compare them in order to choose between them. On the first day he thought that the pleasantness or unpleasantness of the different rates depended somewhat upon his general condition: when he was sleepy, he preferred the slow; when he was wide awake, the faster were the more pleasant.

In series *P* there were 5 undecided, but no negative or doubtful cases; in series *U* there were no negative, undecided or doubtful cases.

(3), (4). *Excitement* and *depression*. During the *E* series, C. wrote: "there seems to be a rate of bodily movement which is natural and easy; the attempt to keep time with rates faster than this natural rate is exciting. Depression is merely the absence of excitement and not itself a positive quality."

"When very slow rates are given, I cannot help dividing the long interval between beats, and counting twice to a beat. In this way I obtain a rate that is twice as fast as that given and much more comfortable." No negative, undecided or doubtful cases were reported in either series.

(5), (6). *Strain* and *relaxation*. To C. strain seems to have meant the muscular tension involved in the effort to "keep up with" the beats. The most straining rates, he says, are those "that require the most muscular tension in the effort to keep time,—those that are farthest from a natural bodily rhythm." These were unpleasant.

Relaxation was merely the absence of strain. Those rates seemed most relaxing that called forth the least muscular exertion and approximated the "natural rhythm." Relaxation was thus not differentiated from depression.

In series *S* there were no negative, 1 doubtful and 4 undecided cases; in series *R*, no negative, doubtful or undecided cases were reported.

SUMMARY OF RESULTS FOR C.

(1) The curves of *P* and *U* take opposite courses. The *P*-curve shows some irregularities; but the *U*-curve is more regular, and both are of the familiar type.

(2) The *P–U* judgments are direct.

(3) The observer shows the tendency to make the faster rates exciting, the slower rates depressing, which we have found also in M. and G. The irregularities at the beginning of the curves are apparently due to the observer's tendency to double the rates.

(4) Strain was unpleasant, and the curves of *S* and of *U* accord. *R* is identified with *D*, and the same type of curve results.

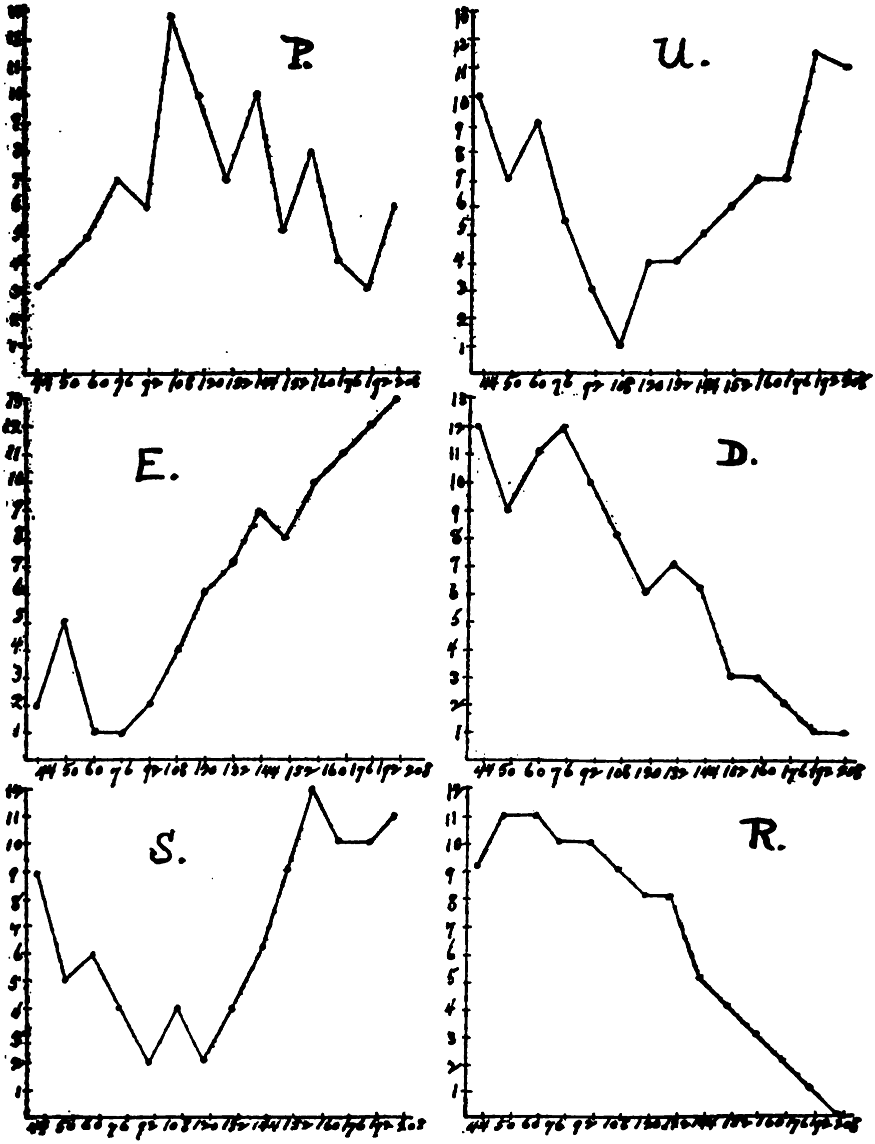
CONCLUSION.

We have now obtained the six series of affective judgments posited by the tridimensional theory of feeling, (1) with clangs, from four observers, and (2) with metronome intervals, from three observers. The results may be briefly summarized as follows.

(1) For all observers alike, and for both kinds of stimulation, judgments of *P* and *U* were easy, direct and natural. It was exceptional to find any reason, any basis, for these judgments: the stimuli were intrinsically pleasant and unpleasant,—more pleasant or more unpleasant than their neighbors: and where a reason or a basis was found, outside of intrinsic affective tone, it lay in the organic reaction set up by the stimulus employed. The curves of *P* and *U* followed opposed courses. There can be no doubt, then, upon any theory of feeling, as regards the validity of the *P–U* dimension.

(2) No evidence was obtained, throughout the investigation, of the existence of a plurality of *P* or of *U* qualities. On the contrary, *P* and *U* appeared to be homogeneous and simple, identical in all experiments. Variations in the 'color' of

PLATE VII.



P and *U* were referred by the observers to variations in the organic reaction set up by the stimulus.

(3) For all observers alike, and for both kinds of stimulation, judgments of strain were easy and, on the whole, direct. Strain was, however, described in 'muscular' terms throughout, and increasing strain meant, uniformly, increasing unpleasantness. The curves of *S* correspond closely to the curves of *U*. So far as its affective side is concerned, therefore, strain may be identified with unpleasantness; there is no evidence of a new affective quality or affective dimension.

(4) Less direct, but still fairly natural, were *E*–judgments upon clangs. In the case of M. and G., excitement means unpleasant muscular tension; and the *E*–curves agree with the *U*–curves. In the case of C., excitement means, according to the nature of the muscular tensions aroused, now the opposite of melancholy, now that of calm: usually it means the latter, and is unpleasant. For W., on the contrary, *E* is usually pleasant, sometimes unpleasant. There is no evidence of a specific *E*–dimension, or of a number of different *E*–qualities. The *E*–judgments upon metronome intervals tend to be purely intellectual (sensationally motived) and not affective judgments. Fast rates are termed exciting, slow rates depressing. High degrees of excitement are found unpleasant.

(5) Still less direct are the *D*–judgments. For M., with clangs, the whole experiment was associatively motived, and the standard of judgment associatively maintained. We may surmise from the introspections that, had M. been asked to judge of the 'soothing' (instead of the depressing) character of the clangs, she would have given a *P*–curve as response to the instruction. For G., *D* means tranquility, soothing calm; and his curve is a *P*–curve. For C., *D* vacillates in meaning: his judgments are either associatively mediated, or judgments of *U* (depression=melancholy). For W., *D* is a still more artificial and still more vacillating term. The mood of depression is associatively aroused, and shows alternating phases of *P* and *U*. In the metronome experiments, *D* has for M. no constant affective value; the judgments are intellectual, and associatively motived. The judgments of G. and C. do not either show any affective influence.

(6) The *R*–judgments of M. with clangs are associatively motived: *R* is much the same as *D*. G., C. and W., on the other hand, make *R* the opposite of unpleasant strain, and accordingly give *P*–curves. In the metronome experiments, the results are the same, but their distribution is different. M. and G. give *P*–curves, making relaxation mean pleasurable muscular attitude or pleasurable organic set; C. identifies *R*

with *D*, and gives a curve similar to his *D*–curve. Nowhere is there any evidence of a specific *R*–dimension or *R*–quality.

(7) The conclusions drawn by Titchener from his two-dimensional study of the Wundtian theory are confirmed by our results.

(8) The tridimensional extension of the method of paired comparisons is justified by the directness of all *S*–reactions and of the majority of the *R*–reactions to clangs. On the other hand, the *E–D* experiments with metronome intervals failed entirely to educe an affective reaction, whether of the familiar *P–U* or of the hypothetical *E–D* type.

Our evidence, then, is against the tridimensional theory of feeling; it supports the dual theory in its traditional form. We have, now, no desire to press this evidence to the breaking-point. We grant freely that the experiments are not even yet very numerous; that the observers also should be increased in number; and that the range of stimuli has been limited. What we urge is, that the experiments, so far as they go, all point in the same direction. And we urge, further, that they are experiments: that our observations have been made under standard conditions, with trustworthy and conscientious observers, and by an approved method which allows of the correlation of subjective and objective results. More than this we do not assert. It may be that future investigation will show our procedure to have been too crude and general to do justice to the full complexity of factors co-operating in the affective consciousness. It may be that that subtle and intangible influence of 'laboratory atmosphere' has, in spite of all our care and of the attempted impartiality of our observers, been at work to vitiate our conclusions. We cannot disprove either of these objections, if it be brought against us. We shall be satisfied if we have shaken the pluralists, for the moment, out of their dogmatic slumber.

For the pluralistic theory may, surely, be described as a dogma. On the experimental side, appeal has been made by its representatives only to the results of the method of expression. This method has repeatedly been declared, by Wundt himself, to be subsidiary to the method of impression. That apart, however, the appeal has broken down almost as soon as made. Or is there, at the present time, any iota of evidence for the multidimensional theory to be drawn from the published investigations? The evidence seems to be found, rather, in the bias of the psychologizing mind. The affective life is extremely rich, extremely complex; and it appears, so to say, derogatory to the composition of the human mind to find the affective factors proper solely in pleasantness and unpleasantness. When James says that the dual theory "is a hackneyed psychological doctrine, but on any theory of the seat of emotion it seems to me one of the most artificial and scholastic of the untruths that disfigure our science;"[1] when Ladd writes of the same theory that it is not only "wholly inadequate to describe and explain the admitted data of consciousness, but even contradictory of those data,"—that it "receives confutation at every point from the data of psychological science;"[2] when Wundt declares that to the observer who has freed his mind of prejudice, and who adopts the right method and principles, there "drängt sich unweigerlich eine Anzahl seelischer Zustände der Wahrnehmung auf, denen man zwar durchaus den Charakter von Gefühlen zuerkennen muss, die sich aber in die Schablone der Lust und Unlust nimmermehr einzwängen lassen;"[3] when Lipps postulates, besides pleasantness-unpleasantness, primary feelings of effort, of certainty, of reality:[4] we can understand the attitude which these authors take up, and we can give a hearty assent to their insistence upon the variety, the delicate shading, the subtle transitions of emotive experience. The question of the plurality of the affective elements *sensu stricto* is, however, a question of fact; and we have a right to demand more than casual self-observation, more even than intense psychological conviction,—we have a right to demand evidence of the combined objective and subjective sort which direct experimentation alone can furnish. So long as no attempt is made to bring such evidence, is not the pluralistic theory a dogma? Do not the differences which the different authors evince in their catalogues of the primary affective dimensions show, with all desired clearness, that the theory is a matter of personal impression and prepossession, rather than of systematic work? And may we not, with justice, challenge the partisans of the theory to take up the issue, experimentally, and at least to withhold their condemnation of the traditional view until some positive evidence for their own has been adduced?—

To us, it has seemed best, instead of extending the method of paired comparisons beyond the point now reached to other classes of stimulus, to change the venue of the problem altogether, and to attack it on the side of *mixed feelings*. A second article will therefore report an experimental study of these much-discussed processes.

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[1] Psychol. Rev., i, 1894, 525.

[2] Psychol., descrip. and explan., 1894, 167, 169.

[3] Phys. Psych., ii, 1902, 285.

[4] Selbstbewusstsein, Empfindung und Gefühl, 1901; Vom Fühlen, Wollen und Denken, 1902.

ACCURACY IN HANDWRITING, AS RELATED TO SCHOOL INTELLIGENCE AND SEX.[1]

ARNOLD L. GESELL.

If Preyer is at all right in saying that "the connection between physical and psychical processes reveals itself more plainly in handwriting than in any other voluntary movement" (18:1), a search for correlations in this direction should be profitable. Preyer's own attempt, along with Crepieux-Jamin's (6) and Meyer's (17), to establish graphology on a scientific basis constitutes such a search for correlations on a very ambitious scale. The more restricted investigations by the alienists (7; 11; 17), with the Goldscheider-Kraepelin *Schriftwage*, may also be viewed as a search for the correlates of handwriting, especially the handwriting of the insane. These two fields, the characterological and the pathological, have so far been cultivated to the comparative neglect of the more normal and commonplace aspects of handwriting.

The present study concerns itself with ordinary school penmanship, considered as a motor function. There are peculiar reasons why the handwriting of a large number of school children should be studied from this standpoint. Writing is a very delicate, complicated activity, requiring a high degree of co-ordination and years of almost daily practice before it becomes an established acquisition. Few movements to which the hand of the ordinary child is trained, are more difficult, and in no other example of "manual training" are the degrees of efficiency so conveniently recorded. Furthermore, in the case of no other school activity is there such a conspiracy of influences to secure uniformly excellent results. Constant pressure from the teacher, usually supplemented by praise or blame from the parent, and a mathematically accurate model are for eight years not allowed to lapse, so that here conditions are most favorable for the production of absolute uniformity. The very apparent and tangible differences which appear in spite of these conditions challenge consideration.

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[1] I wish to acknowledge my indebtedness for various favors to Mr. Homer P. Lewis, Superintendent of Schools, Mr. Joseph Jackson, Principal of the English High School, Mr. Edward M. Woodward, Principal of the South High School, of Worcester, Mass., and to Superintendent Walter E. Fernald and teachers of the Massachusetts School for Feeble-Minded Children. I have also to acknowledge many courteous services from the principals and teachers of the Worcester grade schools, who assisted in the collection of material.

The present study limits itself to the differences in the accuracy of penmanship, that is, the differences in the amount of deviation from a perfect copy. The principal material was collected with the assistance of one hundred and five teachers, from the public schools of Worcester, Mass. Each teacher submitted four sets of representative specimens written by the pupils of her grade, as follows:

Group I. Specimens from the three *Best writers* in the grade.

Group II. Specimens from the three *Worst writers* in the grade.

Group III. Specimens from the three pupils of highest mental ability as represented by school standings.

Group IV. Specimens from the three pupils of lowest mental ability as represented by school standings.

To each specimen was attached a slip giving desired data about the pupil, as indicated below:

(1.) *School Intelligence* (as shown by standings): *Very Good, Good, Fair, Poor, Very Poor.*

(2.) *General Intelligence* (irrespective of standings): *Bright, Average, Dull.*

(3.) *Motor Ability* (a careful judgment of the pupil's muscular dexterity, as shown in Drawing, Sewing, Manual Training and general aptness in using fingers, hands and arms): *Clever, Average, Clumsy.*

(4.) *Facility in Writing: Ease, Moderate Ease, Effort.*

The grades represented in the returns are from I to IX inclusive, with a total enrollment of 4,361 pupils. The average number of specimens for each grade is 140; the number for each *Group* is 315, making the total number of specimens classified and tabulated below 1,260.

	Sex		School Intelligence					General Intelligence			Motor Ability			Facility in Writing		
	Boys	Girls	Very Good	Good	Fair	Poor	Very Poor	Bright	Average	Dull	Clever	Average	Clumsy	Ease	Mod. Ease	Effort
Group I	122	193	86	105	82	30	12	122	168	25	124	174	13	187	115	10
Group II	238	77	26	54	92	88	55	60	160	95	18	124	152	52	103	148
Group III	147	168	236	64	5			269	46		159	146	17	171	111	26
Group IV	174	141			22	123	170	11	97	204	23	149	137	56	141	104

The figures show a surprisingly decided tendency for accuracy in children's writing to vary directly with school intelligence. It should also be mentioned, since the above table gives only the sum totals, that the tendency is a constant one, and presents itself with but slight fluctuations in every grade from the first up. In *Group I*, which contains the *Best writers*, there are 191 *Good* and *Very Good* pupils. Out of a total of 315 in this group there are only 25 (7.8%) who are ranked as *Dull* by their teachers. In *Group II*, which contains the *Worst writers*, the corresponding tendency is just as constant. There are 143 *Poor* and *Very Poor* pupils against 80 *Good* and *Very Good*. The number of *Bright* pupils is 60 (19%). The tendency toward a correlation is corroborated by the manner of distribution of those pupils who fall into more than one group. Out of a total of 1,260, the number of *Best pupils* who are at the same time the *Worst writers* is only 6; and the number of *Poorest pupils* who are at the same time the *Best writers* is only 7. On the other hand there are 66 *Best pupils* who are also the *Best writers* and 70 *Poorest pupils* who are also the *Worst writers*. The general tendency was again verified when the 730 specimens in *Groups III* and *IV* were divided into two equal sets of best-written and worst-written specimens; 207 pupils in *Group III* wrote best; 207 pupils in *Group IV* wrote worst. These same 730 specimens were afterwards taken and subjected to a rigorous sifting process, by means of which the more neutral specimens were discarded and two extreme classes of the most and least accurate writing were formed. Of 180 *Very best* specimens, 125 were written by the *Best pupils*, a direct correspondence of 70%. Of the 134 *Very worst* specimens 103 were written by the *Poorest pupils*, a correspondence of 77%.

As a check and sidelight on these results a collection of specimens from the six grades of the School for the Feeble-Minded at Waverley, Mass., was examined. These specimens, 72 in number, were tabulated and classified on a basis corresponding to that used for normal children. The results are in harmony with those already noticed. Out of 18 *Best writers* not one was reported as *Very Poor* (in school intelligence); while out of 18 *Worst writers*, only one was reported as *Very Good*. The number of pupils falling in both *Groups I* and *III* was 4; the number falling in both *Groups II* and *IV* was 7. The division of the specimens in *Groups III* and *IV* into 2 equal sets of best and worst-written, showed that 13 of the 18 *Best* specimens were by *Best pupils*, and 13 of the 18 *Worst* specimens were by *Poorest pupils*.

Before commenting on the nature and validity of the correlation suggested by these results it is necessary to take into account another factor, which will be seen to have considerable influence on the accuracy of writing; namely, that of sex. The sex differences in writing have already been investigated, notably in the experimental studies by Gross (11: 450) and Diehl (7:1) and in Binet's interesting paper, "*La graphologie et ses révélations sur le sexe, l'âge et l'intelligence.*" Binet believes the existence of sex characteristics in writing "to be demonstrated in a most satisfactory manner" (4:208). These studies, however, are concerned with the handwriting of adults and offer nothing that has an application here.[1]

A few figures will bring out the extent of the sex difference in accuracy of writing. *Group I* contains 193 girls (61%); *Group II* contains 77 (24.6%); *Groups III* and *IV* respectively contain 168 and 141 girls. Of 180 *Very Best* specimens selected from *Groups III* and *IV*, 116 (64.4%) were written by girls; while of 134 *Very Worst* specimens from these two groups, 94 (70%) were written by boys. Again, of 63 pupils falling in both *Groups I* and *III*, 43 (68%) were girls; and of 70 falling in both *Groups II* and *IV*, 52 (74%) were boys. In the high school the differences are most marked. Taking 257 specimens of vertical writing and dividing them according to the uniformity and symmetry of the script into 4 classes, it was found that the most copy-book-like class contained 11 boys' and 43 girls' specimens; and the least copy-book-like class 31 boys' and 7 girls' specimens; that is, the percentage of boys respectively in the most and least calligraphic classes was 20.4% and 82.6%. Again, taking 1,011 papers representing the enrollment of two high schools (445 boys and 566 girls) and discarding 471 border line specimens, we find that of 299 best specimens, 229 or 76.6% are by girls and of 241 worst specimens, 193 or 80% are by boys.

It is evident that we have a sex factor to reckon with here.

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[1] The sex characteristics, whatever they may be, seem to be discoverable to a certain extent below the adult age. The writer made a careful selection of 50 specimens written by high school pupils, with a view of testing the prominence of the difference between boys' and girls' writing; 28 of the specimens were representative, 22 "difficult." Sixteen persons, teachers and students, submitted each to two tests on the set of papers. This made a total of 1,600 judgments; there were only 98 cases of contradiction and 37 cases of no decision. The number of errors was 599 or 37.4%. (The error by mere chance would have been 50%.) 482 or 80.5% of these errors were on the difficult specimens. The average time devoted to each paper on the first test was 12 seconds. Many of the reasons for the judgments, given after introspection, were laden with graphological connotations; but others rested on empirical data such as "I have never in all my experience seen a boy make a *G* that way," etc. The amount and character of the irregularities in the writing were often mentioned in the decision.

It would be difficult to think of any other school activity in which the influence of this factor comes out so strikingly. The usual tests of motor ability, sensory discrimination, etc., reveal only slight differences between boys and girls as a class. Speaking of men and women, Havelock Ellis says, "There are nearly always differences — but these differences are complex and manifold; they do not always agree; they never show any general piling up of the advantages upon the side of one sex or the other." (9.) In the present case, however, there seems to be distinctly such a piling up in favor of one sex.

With the aid of a detailed analysis of the writing process it may be possible to understand the meaning of this sex difference, and also the influence of the other factor of school intelligence. From the start it must be remembered that we are dealing with handwriting during its formative period, when the movements have not yet become fully automatic, and the accompanying conscious processes are different from those which exist with adults. According to Judd (13:243), "Writing, which is essentially a co-ordinated movement, has to be developed trial after trial, with consciousness directed not upon the movement itself, but on the visual images which appear as the results of the movement." "There is no conscious selection of the hand movement," but rather, "it gradually becomes incorporated without any conscious purpose or clear recognition into the total automatic form of movement." "These facts," he says, "make it difficult to attribute to the sensations of movement any important part in the building up of the writing habit or in the maintenance of correct forms of movement, after the habit has been developed. The various factors have been gradually added to each other by a process of organic fusion, not controlled by consciousness. These separate factors are each the result of many trials in which the guiding motives have been, first, the reproduction of visual forms, and second, the avoidance of difficult, cramped positions of the hand." This statement should be taken in connection with Judd's other statement, that the special character of the writing movement is "determined in the main by the influences that are brought into play during the years of practice, which are generally devoted to the acquisition of this art." Such an explanation of the facts reduces this motor function very largely to a mental rather than a physical basis, and makes more probable its correlation with some other function of a psychical order. To quote Judd again, "The individual variations in writing are due to the way in which the visual factors and the factors of movement have been interrelated. If one insists on the constant and clear recognition of the visual pattern he may ultimately conform the movements by a large amount of practice to his pattern." Throughout his whole analysis, Judd emphasizes the importance of these conscious factors. Success in penmanship, or accuracy in the writing movement, is made to depend upon the capacity and willingness to exercise visual control. The results of Scripture, Smith and Brown's study "On the Education of Muscular Control and Power," point in the same direction. From the outcome of 2,000 experiments they felt justified in the conclusions that "steadiness of movement can be increased by practice," and "this training seems to be of a psychical rather than of a physical order and to be principally in steadiness of attention." (19:114.) Similarly, Hall says, "Exactness of movement is one of the chief products of skill and practice, and is probably more indicative of mental development . . . ." (12:144.) Baldwin holds that "The most adequate theory of the mechanism of (motor) control makes it a function of attention." (2:115.)

However dependent accuracy of movement may be on psychical factors, no one would be disposed to argue that handwriting in any way stands for a high order of intelligence. This is shown by the fact that in cases of insanity where nearly all the mental activities have deteriorated, the writing response is sometimes the only coherent response that can be called forth. The specimens of the handwriting from imbeciles are, likewise, often indistinguishable from those of normal children, in the general appearance and finer co-ordinations of the script. Still it would be wrong to conclude that handwriting is possible with even a very low degree of intelligence. Only imbeciles of high grade can ever hope to acquire the art, and Barr classes it as being more difficult than the use of the hammer and chisel. (3:163.) The slow and labored manner in which even high grade imbecile pupils do their writing must be taken into account, as also their great dependence upon a copy to which they can refer. They have considerable difficulties, for example, with complicated letters like *f*. The learning process makes a demand upon their consciousness, so that as one teacher told me, the appearance of writing in the lower grades is a hopeful indication of dawning intelligence. This teacher has also observed that when the imbecile "child first writes, his penmanship is at its best. When the mind is occupied with the expression of an idea, the writing frequently becomes careless and inferior." But when the mind is not so occupied feeble-minded children take a strange delight in the act of writing, which goes to show that its demands upon the attention are not onerous. They beg to be allowed to write for "busy work," and are content to copy from their readers by the hour.

Accuracy in writing, then, indicates a kind of intelligence, rather than a grade of intelligence. From the foregoing analysis of the writing movement we saw that the accompanying consciousness was a visual consciousness. In fact, the chief purpose of the writing lesson and the copy-book is to stimulate this visual control. The learning process is a "visual consciousness of the end;" and the relative accuracy of the boys' and girls' writing movements will depend upon their relative willingness and capacity to maintain such a visual consciousness. The decided majority of teachers report that the girls take to writing more readily than do the boys. The number of girls reported as being painstaking in writing is almost twice the number of boys. The number of boys reported as careless in writing is over four times the number of girls. The number of boys reported as disliking writing is over six times the number of girls.

There is evidently a general difference in mental attitude which reveals itself in the accuracy of the writing. Miss Thompson in her extensive study of the mental traits of sex found among women, "a greater taste for working with the hands." "The greater prominence of the visual consciousness among women," she also says, "is especially marked." (21:166.) This is precisely the consciousness which affects accuracy of writing. Jastrow also found among the feminine traits revealed by his study of association, "an attention to the immediate surroundings, to the finished product, to the ornamental, the individual, the concrete." (10:190.) The greater teachability and diligence of the female sex are often mentioned. Riccardi found upon the examination of several hundred school children of Modena and Bologna, that girls have a greater fondness for manual work. (10:202.) That is to say, girls are likely to have a greater interest in writing; and interest and attention are but two aspects of the same function. In the conflict between the visual consciousness and the kinæsthetic and other sensations, the girls attend more to the visual standards and hence are better writers. A comparison of many examples of the most rapid writing in high school boys and girls, shows greater abandon and less embarassment by visual guides in the former. The boys in general, having a milder interest in form and a more vigorous one in content, are much more inclined to allow the visual patterns to lapse, in favor of kinæsthetic sensational control, to the detriment of their writing. By this reasoning the sex differences in the accuracy of penmanship are attributable to differences in mental attitude.

But to reduce the whole writing activity to mental elements would be to ignore some of the facts. Even Judd mentions the existence of "inherited nervous structures," which furnish a general basis for the writing habit. To what extent does this factor of physical structure influence accuracy of writing? The individual differences in size and proportions of the hand, have been shown to have but slight, if any, importance even in determining the general conformation of the script, and probably have little effect upon accuracy of digital or manual movement. There are general differences between men and women in the length of the hand, index finger and thumb; but these likewise are too small to explain the sex differences in the accuracy of children's writing. It is difficult, in any case, to determine when and to what extent clumsiness has *per se* a physical basis. Some of our data, however, are suggestive on this point. The teachers, it will be recalled, were asked in every case to give a careful judgment on the muscular dexterity of each pupil, designating whether *Clever*, *Average*, or *Clumsy*. They were also asked to state whether the pupil wrote with *Ease*, with only *Moderate Ease*, or with *Effort*. The returns on these points are very uniform. Handwriting seems, in all the grades, to be a fairly good index of the general muscular dexterity of the pupils as judged by the teachers. Only 13 out of 315 *Best writers* were otherwise *clumsy;* and only 18 out of 315 *Worst writers* were otherwise *clever*. (It is worth noticing incidentally that there is a marked tendency for this muscular dexterity to vary with mental ability. Of the 315 pupils in *Group III* only 17 were reported as *clumsy*, and of 315 in *Group IV* only 23 were reported muscularly *clever*.) Of 315 *Best writers* only 10 write with *effort;* of 315 *Worst writers* only 52 write with *ease*. These reports, although they represent no more than the observations and estimates of the teachers, are too uniform to be disregarded.[1] It would be begging the question to say that they indicate the existence of purely physical differences in nervous organization, but they point in that direction. Another fact which points in the same direction, is that not infrequently pupils reported as painstaking in their writing are nevertheless inaccurate writers. Writing does not come with equal ease to all. I have inquired into a number of such cases and have found what one would prefer to call physical explanations. Sometimes the poor writing has been described by teachers who have had other members of the family under observation, as a family trait, which would naturally be interpreted as an inherited physical trait.

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[1] It is obvious, of course, that such a uniformity was to be expected if the teachers in their judgments as to dexterity in general were unconsciously determined by skill in penmanship. How far this was the case it is impossible to say, though other forms of muscular activity were specially enumerated in the request for information.

The results of the investigation of Dr. Macmillan, of the Child Study Department, of the Chicago public school system, are *apropos*. After studying the spontaneous writing of many children, he came to the conclusion that "the slope in writing is more a matter of the child's anatomical structure and muscular co-ordination than the individual caprice on the child's part or instruction on the part of the school." (15.)

A close examination of our data reveals another group of facts which are suggestive along this line. Up to the fifth grade, the number of boys and girls in *Group I* is almost equally distributed for each grade, the total number of boys being 70 and the total number of girls being 71,—practically no sex difference at all in the accuracy of the writing. But at the fifth grade the figures take a sudden shift which is stable for the remaining grades, the number of girls who write accurately being twice the number of boys. This shift is at the age of ten years, when the mental traits distinguishing the boys from the girls begin to be more pronounced; but this is also the age when the physical precocity of the girls over the boys takes its start, and the fact that this advantage continues up to the high school age, while the writing habit is being established may not be without important influence. At any rate the sex differences in writing become more marked after the fifth grade.

The tendency toward inaccurate writing among the boys, however, is as pronounced in the first grade, as in all the succeeding grades, which raises the question whether this tendency is not in part constitutional, structural. Sikorski, the authority on speech defects, found after examining some 10,000 pupils that the proportion of left hand writers is almost twice as great for boys as for girls. He also found by comparing the copy books of boys and girls, that ataxic handwriting was from seven to eight times more prevalent with the former. (20 : 208.) He correlates these facts with the well known truth that boys are about three times more susceptible to speech defects than girls. This is a neurological susceptibility, that may extend over to the writing centre which stands in close relationship to that of speech. It is not improbable that the inveterate tendency of many boys toward inco-ordinated writing has a physical background.

Havelock Ellis found stammering and other speech and voice defects and nervous derangements in general common among his British men of genius. He classes bad handwriting along with these defects and approvingly quotes Goodhart, who calls illegibility a disease. Says Ellis, "Illegible handwriting is mentioned in nine cases which certainly need to be increased. . . .

A tendency to scrawly or illegible handwriting has been frequently noted among men of genius of many countries, and is by no means due to too much writing, for it is often traceable at an early age." (8:200.)

All these facts point to possible physical differences, the full importance of which it is impossible to evaluate when it comes to correlating handwriting with other functions. So far as the results of this study go, they emphasize the prepotence of the psychical factors. The influence of differences in intelligence has been noticed, and making some allowance for differences in nervous organization, the influence of the sex factor may be safely attributed to differences in mental attitude. The fact that other studies have shown such slight differences between boys and girls in motor ability tests, and then in favor of the boys (1:193; 5:123), gives weight to this view.

In the end, it is difficult to decide between the primacy of the two factors of sex and school intelligence. In the writing of the high school period the former factor comes out strongly; but an examination of the standings of over 500 pupils shows the latter factor, while present, to be much weaker than in the grades. On the other hand, in the School for the Feeble-Minded where the sexes are segregated, the differences in school intelligence were alone important enough to influence the results. Again, when we take, for example, the minority of 122 boys' specimens in *Group I* and classify them on the basis of the mental ability of the writers, there is a decisive piling up of the specimens in the best scholarship class. Likewise, we have already observed, that below the fifth grade, the distribution of best specimens was almost equal between boys and girls. This fact, however, did not prevent in these grades a marked tendency of the best writing to accompany the higher school intelligence. Individual cases which do not fall under the general rule, and, indeed, often strikingly contradict it, are not wanting either in the high school or the grades. *Accuracy in writing stands for a certain kind of intelligence, and what our results mean is that for a large number of cases, this particular kind of intelligence is more prevalent in girls and in pupils above the average in school standing.*

One other result of the study remains to be mentioned. It has already been observed that painstaking qualities did not always produce accurate writing. In view of the common assertions about manual training, it is interesting to inquire further whether painstaking or careless qualities in a motor function like writing bespeak the same qualities in other school work. Sixty-five pupils (45 boys; 20 girls) were mentioned as careless in writing but painstaking otherwise; 42 pupils were reported as painstaking in writing and careless otherwise.

But these 107 cases, in the minds of the teachers reporting them and in the light of additional figures, are exceptional. In the 917 remaining reports, painstaking or careless qualities in writing bespoke similar qualities in general school work. That is, in 1,022 cases reported as clear instances, there is on this point a direct correlation of 90%. This correlation is so fundamental that it shows apparently no dependence on the three factors considered in this study: school intelligence, sex differences, and accuracy in writing.

SUMMARY.

1. For a large number of cases, accuracy in the handwriting of pupils of elementary grade tends to vary directly with school intelligence.
2. From the fifth grade up through the high school, girls as a class write more accurately than boys.
3. Boys as a class show a greater tendency toward incoordinated writing as early as the first grade and up through the high school.
4. The sex differences in writing become marked about the age of ten, and are largely attributable to the mental factors.
5. If handwriting is an index, painstaking or careless qualities in a motor function bespeak, in pupils of elementary grade, the same qualities in general school work.

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THE EFFECT OF MUSIC ON THORACIC BREATHING.

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(From the Psychology Laboratory of Wellesley College.)

This brief report is an addendum to the paper on "Attention and Thoracic Breathing" published in this *Journal* for July, 1905. The experiments here taken into account were made in the academic years 1903-1904 and 1904-1905 with the assistance of Dr. Hamilton C. Macdougall, head of the Wellesley College Department of Music. The purpose of the investigation was to find traces of any correlation which may exist between the emotional effects of different kinds of music and the varying features of respiration. Certain data furnished by the experiments of the first year have already been incorporated in the earlier paper. The attention and the music investigations necessarily overlap, since listening to music may be regarded not only as the possible condition of emotion but as involving a certain kind of attention. The music experiments of the first year constitute the third group of the attention experiments, and were, with the exception of the control experiments on the dog A., the last group to be made in that investigation.

In the earlier paper the reader will find (1) an account of the apparatus employed in these first music experiments (pp. 272-273) and (2) a discussion of the assumption on which the numerical results for different subjects were massed (pp. 274-275). Throughout the experiments of both investigations, virtually the same apparatus was used, rates of breathing were computed in the same manner, and the results of individual subjects were massed on the same principle. The two investigations diverge, however, in the method by which changes in the *form* as distinct from changes in the *rate* of breathing were estimated In the first year the determinations of form-changes were on various accounts very unsatisfactory. Some slight notice was taken of them in the earlier paper but they will be thrown out entirely here.

In the second year one or more experimental sittings were entirely allotted to determining the average normal breathing-rate of each subject. (On this use of the word "normal," see

p. 267 of the earlier paper.) The tracings thus obtained served as the primary standard of reference for determining the changes in the form of breathing (that is, in regularity, in amplitude, and in the length of the expiratory pause) which took place as the subject listened to music. In the latter half of this period a normal tracing was also taken at the beginning of each music-session to serve as a control upon changes in breathing due to alterations from day to day in the subject's mood and physical condition. At best, even in the second year of the music experiments, the determinations of changes in form were a matter of very rough estimate, and it would be scarcely worth while to give the statistics if their testimony were not upon certain points so unequivocal.

The subjects of the experiments, twenty-nine in number, were all young women, were all, with one exception, college students, and with two exceptions had no greater knowledge of music than is common in educated persons. Only four of the twenty-nine had had more than one year's work in psychology. Only one knew the exact purpose of the experiment. Every experimental sitting was limited to three-quarters of an hour. The music was furnished by Dr. Macdougall and was played upon the organ of the college chapel, an instrument of three manuals and thirty-nine speaking stops.

The problem of the investigation was two-fold. (1) Throughout the work the experimenters sought especially to discover differences in the effect of music in the major and in the minor keys. This variation in the stimulus was made not with any great expectation of finding such differences but in the hope that some other significant correlation might appear in the process of following the guiding-thread which came first to hand. (2) During the second year's work the tracings were studied to determine also the differences in effect between loud and soft music.

According to the music employed, the work falls into four periods. The results of each period can be most quickly understood if stated in close connection with the description of music used. For the sake of brevity, the discussion of results will be reserved till all are presented. The first three periods belong to the first year, and as noted above only changes in rate will here be discussed.

During the first period, eight subjects served for two sittings each. The music consisted of hymn-tunes. These were (1) the familiar tunes "Vox Dilecti" and "St. Andrew of Crete" and (2) two tunes in less ordinary use, "Warren" and "Nightfall."[1] In the first two tunes, there is a sharp transition from minor to major. Each of these two tunes was played three times over, once as mechanically as possible with the minor and major passages at the same rate, once mechanically but with the major faster than the minor, and once with as much contrast between the two passages as could be secured by alteration of stops, tempo, and nuance,—the minor plaintive or solemn and the major triumphant. "Warren" and "Nightfall" were, in general, played twice both in the major and in the minor keys and with such differences in stops and tempo as to make the major lively and the minor mournful. A relief or relaxation-tracing (see page 273 of the earlier paper), not exceeding one minute in duration, was interpolated between two renderings of a melody and sometimes succeeded the last rendering. For this period the average normal breathing-rate was 16.7 inspirations per minute (there were 68 observations of about 3 minutes each, and the mean variation was 3.4); the average relief rate was 17.3 (obs. 69; M. V. 3.2); and the average rate for all the major passages was 18.9 (obs. 67; M. V. 3.8); and the average rate for all minor passages was 17.8 (obs. 65; M. V. 3.6). For all major passages mechanically played, except those in which the rate was noticeably faster than in the preceding minor, the average rate was 18.9 (obs. 23; M. V. 4.5); for the minors corresponding to these majors, the average rate was 17.8 (obs. 19; M. V. 4.3); for all the "relatively fast but mechanical majors," 19.3 (obs. 16; M. V. 4. 7); for all the "relatively slow but mechanical minors," 17.4 (obs. 16; M. V. 3.8); for all "lively majors," 18.6 (obs. 28; M. V. 3.4); for all "doleful minors," 18.1 (obs. 30; M. V. 3.1).

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[1] See in the hymn-book "In Excelsis" (The Century Company), numbers 231, 606, 99, and 100.

During the second period twelve subjects served for one or two sittings each. None of these individuals had served in the first period. The music consisted (1) of the choral in D major from Mendelssohn's Organ Sonata V and (2) of the choral in *D* minor from Mendelssohn's Organ Sonata VI. These chorals are severe in their style. Each was played mechanically five or six times for each subject. The same stops and tempo were used in both. First one was played two or three times, then the other, and so on. Relief tracings were interspersed among the music tracings as in Period I. The average normal breathing-rate for this period was 16.9 inspirations per minute (there were 48 cases and the mean variation was 4.5); the average relief-rate was 16.8 (obs. 86; M. V. 3.3); the average rate for all major passages was 18.2 (obs. 62; M. V. 3.6); and the average rate for all minor passages was 16.8 (obs. 65; M. V. 3.0).

During the third period of the experiment, six subjects served from three to six sessions each. All of these individuals had served during either the first or the second period. The music consisted of the following long compositions: (1) Gounod's "Funeral March of the Marionettes," (2) Rubinstein's "Torch-light Dance of the Brides of Cashmere," (3) The Dead March in Handel's "Saul," (4) Congerin's "Soeur Monique," (5) Handel's "Sarabande," (6) Chopin's "Prelude in D Flat" (7) the Allegretto from Mendelssohn's "Hymn of Praise," (8) Beethoven's "Andante in F," (9) the Adagio and March from Handel's "Occasional Overture." These pieces were used in order, as many with each subject as the time permitted. Each composition was, if necessary, cut, that it might occupy a time no longer than six minutes. Otherwise, it was played properly, and for all subjects, with the same registration and tempo, and so far as possible with the same variations in loudness. In general, each tracing which corresponded to an unbroken major or to an unbroken minor passage represented one case or observation although the "passage" might include the entire composition. In a few cases, however, the long tracings corresponding to the "Dead March" and to "Soeur Monique" were treated fractionally. For this period, the average normal breathing rate was 18.3 (obs. 76; M. V. 2.8); the average rate for all majors was 18.3 (obs. 85; M. V. 5.0); and the average rate for all minors was 17.2 (obs. 70; M. V. 4.9).

In the fourth and last period, which included the whole of the second year, ten subjects served, in general for five sittings each. Two of these sessions were occupied in obtaining records of normal breathing. Only one subject of the second year had served the year before. The music list was as follows: (1) Mendelssohn's Overture to Ruy Blas," (2) Schubert's "Moment Musicale," (3) Beethoven's "Slow Movement for the 7th Symphony," (4) Morandi's "Bell Rondo," (5) Guilmant's "Organ Sonata No. 2," (6) d'Evry's "Meditation," (7) Handel's overture to the "Occasional Oratorio," (8) (9) (10) Boëllman's "Suite Gothique," 1st, 2nd, 3rd movements, and finale, (11) Tschaikowsky's "Sweet Reverie," (12) Silas's "Organ Fantasie in D Minor," (13) Bennett's "Barcarolle," (14) Gounod's "Marche Militaire," (15) Spinney's "Berceuse," (16) Batiste's "St. Cecelia Offertoire in D Minor," (17) Suppe's "Overture to Poet and Peasant," (18) Chopin's "Prelude in D Flat Major." The principle of selection differed in the music of the third and fourth periods. The former had been chosen to present a large variety of major and minor passages; the latter was chosen to present the utmost variety of general æsthetic effect. Each piece was played in full and with feeling and, so far as possible, in the same manner for different subjects. The whole list was not, however, used with all the different subjects. Three individuals heard the first fifteen pieces in order, but numbers 3, 4, 6, 7, 8, 9, 10, and 12 were soon dropped, as on various accounts unsuitable, and numbers 16, 17, and 18 added in their stead.

The experimenters committed two inadvertences in computing the breathing-rates for this period. The value of the experiment, however, does not justify working over the pneumograph tracings a second time. In the first place, the long records of normal breathing, which represented more than three-quarters of an hour, were not treated fractionally; on the contrary the total number of inspirations in these records was divided by the total number of seconds. Of course, normal rates so obtained cannot properly be compared with the normal rates of the first three periods since these were obtained from tracings not more than three and a third minutes long. In the second place, all the tracings for any one subject which corresponded to one given kind of music-stimulus (loud major, soft minor, or what not) in any one musical composition were treated together in series,—*i. e.*, the total number of inspirations was counted and divided by the total number of seconds. Thus each observation of a stimulus rate represents in Period 4 the total rate for that variety of passage (for all loud majors, for instance) for one subject in one whole composition. On the other hand, in Period 3 each observation of a stimulus-rate represents a single passage of the varieties of music to be compared (major and minor). So, also, even in this same Period 4 now under discussion, each observation of a form-change answers to a single passage of loud or soft major or minor music, although each observation of a breathing-rate answers, as just noted, to a series of such passages.

For this fourth period the average normal breathing-rate as computed from the short (three minute) normal tracings was 18.7 (observations, 12; M. V. 3.5); the average normal rate as computed from the long tracings (which often represent a condition bordering on drowsiness) was 16.5 (obs. 12; M. V. 3.4); the average rate for all loud major passages was 21.8 (obs. 59; M. V. 3.9); for all soft major passages, 20.8 (obs. 81; M. V. 3.5); for all loud minors, 21.9 (obs. 42; M. V. 3.8); for all soft minors, 21.2 (obs. 57; M. V. 3.8); for all majors, 21.3 (obs. 140; M. V. 3.6) for all minors, 21.5 (obs. 99; M. V. 3.8); for all loud passages, 21.9 (obs. 101; M. V. 3.9); for all soft passages, 20.9 (obs. 138; M. V. 3.6).

The changes in the form of breathing observed during this period are summarized in the following table which is modeled after Table I of the earlier paper.[1]

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[1] In this table the word "revery" was inadvertantly omitted in the second column opposite the seventh line of figures.

TABLE

Showing changes in the form of breathing when listening to Loud and Soft Music in the Major and Minor Keys.

KIND OF MUSIC.	Number of observations.	Changes in regularity of				Changes in amount of			
		Amplitude.		Expiratory Pause.		Amplitude.		Expiratory Pause.	
		Increase.	Decrease.	Increase.	Decrease.	Increase.	Decrease.	Increase.	Decrease.
		Cases %	Cases %	Cases %	Cases %	Cases %	Cases %	Cases %	Cases %
Loud Major.	151	34.4	37.1	15.1	32.9	8.3	70.6	13.4	69.0
Soft Major.	152	38.2	32.2	13.0	27.8	10.2	75.8	16.2	66.2
Loud Minor.	64	37.5	45.3	12.9	35.5	16.1	71.4	10.9	67.3
Soft Minor.	92	47.8	23.9	26.4	12.6	7.3	74.4	15.9	55.7

All the numerical results have now been presented. Before attempting to draw conclusions from them, it is necessary to note the introspective testimony of the subjects. In the fourth period, each subject was asked after every piece of music whether she had listened; if so, whether she had liked the music; and if she had not listened, what she had thought about. The same questions were asked less systematically in the third period. The replies showed that the subjects, on the average, paid fair attention and derived mild entertainment. On the whole, however, this attempt to secure æsthetic emotion under laboratory conditions appeared to fail flatly. In some very few cases, the subjects were "actively" engaged in trying to recall the name of the composition.

The following conclusions may be drawn from the numerical and introspective data now in hand: In the first place, it is evident from the table and from the rate-averages, that listening to music, loud or soft, major or minor, tends to shorten the expiratory pause and to make the breathing faster and shallower. These are effects characteristic of non-emotional mental "application."

In the second place, it is clear that the music-stimuli did not show any well marked tendency to make the breathing either more or less regular than the normal. The number of cases in which each change occurred fell short of fifty per cent. with every class of stimuli included in the table.

In the third place, no remarkable difference appears in the effect of either loud and soft or of major and minor music. Such slight and possibly accidental differences as do appear will be pointed out in detail. They are as follows: (1) The loud music had more effect than the soft (a) in shortening the expiratory pause and (b) in accelerating the breathing. See (a) the table of form changes and (b) the rate-averages for Period 4. (2) The loud music had more effect than the soft in making the breathing shallower. See the table. (3) The loud music, if it affected the regularity at all, tended to decrease it. The soft minor music, if it had any effect on regularity, tended to increase it. The soft major music tended to increase rather than to decrease the regularity of the amplitude and to decrease rather than to increase the regularity of the expiratory pause. See the table. (4) The music in the major key had more effect upon the length of the pause than had the music in the minor key, not only shortening it but lengthening it in a greater number of cases. See the table. (5) The major passages had more effect in accelerating the breathing than had the minor passages. See the rate-averages for Periods 1, 2, and 3. The slight excess of the average for minors over the average for majors in Period 4 is doubtless due to the fact that the majors lengthened the expiratory pause in a significant minority of cases. On the other hand, the averages for minors in Periods 2 and 3 constitute exceptions (the latter a serious exception) to the accelerating effect of majors in general.[1]

The results thus summarized may perhaps be interpreted in the following fashion: It is clear that in listening to music the breathing tended to assume two of the features characteristic of mental application, namely, rapidity and shallowness, but did not tend to assume the third, namely, regularity. The explanation seems to be that the music, and especially the loud music, had sufficient intrinsic or "primary" value for attention to raise it to a fairly high level but not to keep it steady at that level. That is to say, the music attracted attention without steadily absorbing it, so as to blur the consciousness of surroundings or to inhibit suggested trains of thought.[1] The irregularity of breathing is thus to be interpreted as the symptom of revery. (See page 282 of the earlier paper and its quotation from Mosso.) It certainly was not sufficiently marked to indicate emotional disturbance. In the case of the loud passages, to be sure, the greater tendency toward irregularity and smaller tendency toward shallowness hint at occasional organic disturbance. This disturbance or "shock" is, however, sufficiently explained by the mere sensational intensity of the stimuli. It should be noted that the cases of irregularity cannot be explained by variations in rhythm or loudness *during the course* of a given stimulus. This theory will not serve because each observation of irregularity corresponds to a natural division in the music, a passage of fairly homogeneous character as to loudness, rhythm, and so on. To reiterate, the interpretation offered is that the breathing became more rapid and more shallow than the normal breathing because the level of attention rose, but did not become more regular because the stability of attention did not correspondingly increase.

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[1] The results of the first three periods have been divided into cases of "slow" and of "rapid" breathing according to the average *normal* breathing-rate of the subjects from whom they were obtained. It has been found that in the case of rapid breathing, the average rate for all musical stimuli, taken together, fell slightly below the normal rate, whereas the average stimulus-rate for slow breathing rose slightly above the normal. (See page 291 of the earlier paper.) This showing accords with the general conclusion of the earlier paper that when the level of attention rises, the rate of breathing which has been faster than the average normal is much less accelerated than is the rate of slow breathing and may even be retarded. (See pp. 275 and 291.) It has not seemed worth while in the detailed interpretation of the music results to maintain this division into cases of slow and of rapid breathing. On the one hand, the mean variation in the normal rates of individual subjects is very large (see page 274 of the first paper), and on the other hand, without reference to individuals, the division in question cannot be made in this class of experiments because every stimulus-tracing has not a normal tracing of its own. (*Cf.* page 289 of the first paper.)

One may now turn to the difference in effect between major and minor passages. The fact that the major passages accelerated the breathing more than did the minor passages may be explained, at least in part, by differences in the absolute time-rate at which the two were played and by differences in associations, verbal or non-verbal This is the obvious interpretation of the breathing-rate averages of the first period. The difference between the effects of the slow minors and fast majors is especially noteworthy. But in the familiar hymn-tunes "Vox Dilecti" and "St. Andrew," even when both the major and minor passages were played mechanically and at the same rate, the major had the greater accelerating effect. This is doubtless to be explained by such verbal associations with the majors as "My thirst was quenched, my soul revived," and "Christian, up and smite them!" In the second period, that of the two chorals, the subjects probably paid less attention to the minor than to the major which was more familiar. By their own testimony, they were little impressed by either. Hence, perhaps, the anomalous result for the minor choral which, on the average, failed to accelerate the breathing at all. In the third and fourth periods, verbal associations were ruled out and the major passages were not more familiar than the minor. No data exist, however, for determining whether the difference in effect may or may not be explained by differences in tempo, in rhythm or in registration. As noted above, each major or minor passage was played once for each subject, and in the same manner for all subjects; variations in rate, rhythm and stops were not made in the playing of the same piece. In short, some slight essential difference in the effect of major and minor passages is neither proved nor disproved by this investigation.

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[1] It will be remembered that Binet and Courtier found irregularity of breathing in somewhat similar experiments upon a single subject. These experimenters attributed it to "le développment des idées et des sentiments qui trouble la respiration, tout en tenant compte des effets spéciaux dus à la mélodie et à l'harmonie." (L'Année Psychologique, pp. 114–115.) In the present experiments, it is probable that full-bodied emotions were much less numerous than wandering trains of thought.

The present study of the effect of music upon breathing has scarcely broken ground and has been suspended as requiring too prolonged and laborious co-operation on the part of an expert musician. It is, none the less, the opinion of the experimenters that a systematic numerical study might profitably be made of the changes produced in the rate and rhythm of breathing by variations in the rate and rhythm of music.

PSYCHOLOGICAL LITERATURE.

Psychology: An Introductory Study of the Structure and Function of Human Consciousness, by JAMES ROWLAND ANGELL. Henry Holt & Co., New York, 1904. pp. vii, 402.

The prompt and generous welcome already accorded Professor Angell's book, both by psychologists and teachers of psychology, removes the necessity for a review, in the primary sense of the term. The writer will therefore pass at once to matters of appreciation and criticism.

The *Psychology* gives evidence of two broad lines of influence, one of which, at least, all writers on general psychology since the eighties have felt as a constant inspiration and have acknowledged as a common debt. The first influence derives from the earlier writings of William James, and the second may be traced to the widespread tendency to apply to psychological problems the principles and the methods of organic evolution. The two influences are, by no means, entirely distinct; for *The Principles of Psychology* reveals a keen and irrepressible interest in physiology, and it tends, moreover, constantly to view the human mind in the light of the needs and functions, past and present, of the animal organism. The more specific attempt, however, to rewrite psychology in terms of biology may be traced to a protestant movement, both within and without psychology, which has been particularly active in the last decade. As the first general result of this movement, Angell's *Psychology* bears a special significance.

The author's standpoint and its distinction from physiological psychology are plainly set forth in the following passages taken from the introductory chapter. "Psychology takes for itself a certain definite domain, *i. e.*, consciousness as a life process." "In our study of mental processes we shall adopt the biological point of view just now dominant in psychology, and regard consciousness . . . . as one among many manifestations of organic life, to be understood properly only when regarded in connection with life phenomena." "Our adoption of the biological point of view . . . . will mean not only that we shall study consciousness in connection with physiological processes wherever possible, but it will also mean that we shall regard all the operations of consciousness . . . . as so many expressions of organic adaptations to our environment. . . . To the biologist an organism represents a device for executing movements in response to the stimulations and demands of the environment." "Mind seems to be the master device by means of which these adaptive operations of organic life may be made most perfect. We shall consequently attempt to see in what particulars the various features of consciousness contribute to this adaptive process."

The two kinds of psychology, structural and functional, are now generally recognized; but the author's assertion that "psychologists have hitherto devoted the larger part of their energy to investigating the *structure* of the mind," will come as something of a shock,—especially to those psychologists who have looked in vain through the history of the science for a consistent and systematic account of consciousness from the 'structural' point of view. It should be said, however, that the author seems to take the term not in the more usual sense of James's original distinction between structure and function (*Mind*, O. S., IX, 18–19; *Prin.* I, 478), but, negatively, as *any* aspect of mind not included under *organic* function.

It is a curious fact that psychology has resisted, so far as it has, the allurements of organic evolution. Ethics, sociology, anthropology, and even the history of religion, seem to have been more profoundly modified by the 'biological method.' Certain writers—notably English and, to some extent, French psychologists—and isolated problems, such as the nature of emotion and of space-perception, have, it is true, strongly felt the influence; but a concerted movement in the direction of biological interpretation of the adult human consciousness has only recently been organized. This movement must, of course, not be confounded with 'genetic' psychology of the ordinary type. Ever since Darwin, the developmental psychology of the child, of animal forms, and of the race at large, has had a steady following; but, until recently, no one except Herbert Spencer has, so far as I recall, succeeded in writing a general account of the typical human mind in terms of current biology. Spencer's *Principles* was ably and skillfully done, and it has, without doubt, exerted a strong influence upon popular thinking; but we must remember that it was done over fifty years ago and that it therefore escaped the vast burden of facts that is laid upon more recent writers of psychological text-books and treatises.

The fundamental question is, what does the 'biological point of view' yield for psychology? It may, without hesitation, be conceded that the point of view furnishes a short-cut to 'human values,' and, therefore, to the various fields of psychological application. Mind is, for the biologist, an 'activity,' and it is, as Angell says, "mental activity, rather than mental structure, which has immediate significance for thought and conduct." More than this, it is a subject of common remark that various dependent sciences and arts have grown weary while waiting to share in the benefits of psychological research. In some instances they may be said to have waited on the strength of promises, overt or implied, in other cases sheer want seems to have counseled patience, while again, the psychologist's confidence in his own methods and his distrust in the practical utility of older systems may also have served to maintain high expectations; but, on the other hand, it must not be forgotten that hasty and rash applications, gross ignorance of method, and an underestimation of the complexity and the subtlety of mind have, in the hands of 'practical' persons, played their large part in deferring and in defeating hope. Let the fault fall where it may, great expectations there were that have not been fulfilled. The result is natural. "Patience is a composing but lean dietary," and it seems, in the present case, likely to be exchanged for what at least promises to offer a more substantial ration.

How far psychology should look outside and beyond itself, for ends and motives, is difficult to decide. It is impossible for any science to strike a steady balance between self-love and benevolence. The history of organized knowledge shows a constant interplay of the two springs of action. The world's needs have, in turn, fostered, and been fostered by 'pure science." Purity' itself has meant both productiveness and sterility, germinative power and seediness, academic breadth and cloistered formalism. Training, method and undivided purpose are of greater importance than the end. As Herbart somewhere puts it: "*Geduld und frischer Muth ist die Hauptsache.*"

Whatever its achievements for thought and conduct, the biological point of view in psychology is to be criticised on the score both of precision and of method. What it gains in the solution of practical and specifically human problems, it loses in its disregard for carefully collected facts.

The psychological research of the last twenty years is, for example, in the work under discussion, mentioned with striking infrequency. Experimental work, in particular, is almost wholly lacking. This is explained, perhaps, by the fact that the psychology of 'mental activity' regards 'experimental psychology,' not as a *method*, to be used wherever possible in careful observation, but as a co-ordinate *branch* of the science,—a branch which may, therefore, be largely neglected in a general 'introductory study of human consciousness.'

In place of concrete experimental data, the method discusses the 'nature,' the 'salient characteristics,' the 'distinguishable features,' the 'phases' of mental experiences or mental 'operations,' supplementing its discussion of these 'more important elements' of consciousness by tracing "the genesis and function of the process in the individual or the race."[1] Thus sections are devoted to the definition, the analysis, the genesis, the development and the general function of perception, to the definition, the analysis, the genesis and function of imagination, etc. The most characteristic part of the treatment is the more specifically 'functional' part. But the functions are described in quite general and, for the most part, superficial terms. They seem, to the present writer, to have a popular (or at most a biological) rather than a psychological value. I give illustrations. "The first and basic function of perception, then, is to afford us our primary knowledge of a world of objects amid which we have to live." "The function of sensation is to furnish us with the elementary symbols of the various things in the world about us. . . ." The 'acts' involved in emotion and instinct are or were 'means toward the realization of some end,' and attention is a discriminating and combining faculty that performs the 'actual work of accommodation' in the adjustments of the organism. Similarly, the genetic references are, as a rule, vague or general; they are made, *e. g.*, to 'the new-born babe,' or 'the outset of life,' or 'the young child,' or 'the infant consciousness,' to the 'animal consciousness' or to 'racial experiences.' If genetic psychology is to furnish the pattern for general psychology, surely the best and the most accurate genetic psychology is none too good! A science may well hesitate to adopt a method which comes to its chief issue in naïve generalizations partly common to all psychologies and partly borrowed from a neighboring science.[2]

The primary difficulties of the method are two. First, the method involves a contradiction. It assumes that mind is an evolutionary agent, that, 'consciousness tends to appear where the reflex and hereditary responses of the organism are inadequate to cope with the demands of the environment;' that 'consciousness is a systematizing, unifying activity;' that mind is 'an engine for accomplishing the most remarkable adjustments of the organism to its life conditions.' At the same time, the method warns us against believing that, in one form of nervous action, 'the mind suddenly produces changes in the nerves' (*sic*), while, in another form, it does not; and the reader is told most positively that to say: "the mind might in a wholly unique manner step in and bring about changes in the action of the nervous system" is but to use metaphor. Now consciousness is either a cause

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[1] It should be pointed out that the terms 'process' and 'element' are used in the book in a quite untechnical sense.

[2] An alternative mode of introducing evolutionary arguments and conclusions, into psychology, is by way of illustration and corollary. Sully, *e. g.*, considers, in fine print and foot-note, the biological interpretation of attention, action and feeling. (J. Sully: *The Human Mind*, 1892, I, 78; II, 203.)

(as natural science uses the term), or it is not. If it is, it needs no apology; if it is not, it *has* no 'biological function,' and its use as a cloak to cover the biologist's ignorance of evolutionary processes is unjustified. Functions mind may still have, but not of the biological sort.

Secondly, the method gives no systematic account of its functions. Consciousness, it affirms, is full of 'acts' and 'activities;' but it fails to organize these. Attention is, to be sure, an 'organizing activity,' engaged in the 'double process of pulling apart and putting together;' but interest, also, 'represents the spontaneous, dynamic side of our psychical make-up;' and, again, the self is 'the knower annealing the various elements of our experiments into some sort of unity,' while 'will' is 'the whole mind active.' What, now, are the interrelations of all these sovereign powers? and how are attention, interest, and the rest, related to such functions as perception, reasoning and imagination, and also to *their* functions? Some principle of classification and subordination there must be in a psychology which treats consciousness as organic; something better, I mean, than the traditional knowing, feeling and willing, or the 'three ultimate modes of being conscious of an object.' Is it to be found in the work which consciousness does? in the objects with which it has to do? or in the natural history of the developing mind itself? Surely, a definitive account of mental functions and their genesis cannot be given without some principle which shall insure organic coherence and organic unity to the multitudinous 'activities' that mind reveals.

I have already referred to the wide welcome which Professor Angell's text-book has received. This welcome is sufficient evidence, not only that a book of the character was needed, but also that the author has been successful in his attempt to satisfy the need. Its sanctions for success are not hard to find. Its author is both psychologist and teacher; it is well and attractively written; it fills a place not otherwise filled in the literature, and it appeals to a dominant interest in the student of life and of human nature. Where I have criticised it has been criticism of standpoint and of method. I have only praise for the clear, straightforward and sincere development of the doctrine of consciousness considered as a phenomenon of life.

I. MADISON BENTLEY.

Dictionary of Philosophy and Psychology, written by many hands and edited by James Mark Baldwin. Vol. 3. Bibliography of Philosophy, Psychology and Cognate Subjects, compiled by Benjamin Rand. The Macmillan Co., New York; Macmillan & Co., London. In two parts. 1905. Price $10. pp. xxv, 542; vii, 543-1192.

The long-awaited bibliography which forms the third and concluding volume of Baldwin's *Dictionary* appears in two parts, bound uniformly with the first two volumes, approximating them in bulk (1192 as against 1536 pp.), and costing together as much as they cost at the date of their first issue. A Prefatory Note by the general editor states that the bibliographical lists extend to 1902. The compiler's Preface indicates the sources, aims and scope of the work.

"The book as a whole is entitled a 'Bibliography of Philosophy,'"—so the compiler begins, in opposition to his title-page,—"and it will be found to comprise a series of bibliographies, including the History of Philosophy, Systematic Philosophy, Logic, Æsthetics, Philosophy of Religion, Ethics and Psychology." "Between the methods of a complete compilation and of a descriptive commentary of philosophical works a golden mean has been sought. On the one hand the bibliography, though not exhaustive, aims to be comprehensive in its scope. From beginning to end the endeavor has been to preserve in the amount of material as governed by a work of the present size the relative importance of authors and subjects. On the other hand the absence in the bibliography of explanatory comment on books has been compensated for, it is hoped, by the numerous references to important critical reviews of them by specialists." "Readiness of use, especially for the uninitiated, is an essential in a bibliography. For this reason the philosophers . . . have here been brought together in a single alphabetical order. Likewise in the various systematic bibliographies the authors under the systems and special topics, as well as these topics themselves, will be found in alphabetical arrangement." The work contains the titles of "possibly sixty thousand or more volumes and articles."

One can have nothing but sincere admiration for the pluck and determination with which the compiler has carried through his stupendous task. He has carried it through, apparently, without clerical assistance, without financial support, and practically without expert advice. "More than a decade" of "single-handed and self-supported endeavor" has been spent upon it; "the entire work has been accomplished in the Harvard University Library;" the courtesy of whose officers is acknowledged in a foot-note; and the same foot-note limits the expert aid to "suggestions on classification received from Professors J. Royce and H. Münsterberg, and linguistic corrections from Professor L. Wiener." Truly, a stupendous task,—and a task whose reliable performance must be of inestimable benefit to students of philosophy and psychology. I know, only too well, the amount of time and trouble required for the making of even a very minor bibliography; and, looking at these two volumes, I am inclined to withhold every word of criticism, and simply to express appreciation of Dr. Rand's achievement.

Nevertheless, criticism must be called to its work; for the bibliography is to be used. In the following notes I have confined myself almost entirely to *Bibliography G, Psychology*, the concluding section of the whole: it is only for this section that I can claim competence.[1] This restriction is, however, the less important since the compiler declares that "a bibliography is a growth, and in fullness of detail and in accuracy this work will be found to improve as it advances." We may, therefore, fairly take the final lists as a sample of the work at large.

What are Dr. Rand's canons? "The names of philosophers are printed in full and in the language of the country to which they belonged" (*sic*). I turn to Wundt (535), and I find "Wundt, Wilhelm." Again: "The titles of books are printed in full in the language in which they first appeared." Did, then, Berkeley publish in 1709 a work entitled "Theory of Vision" (120,926)? Did he publish in 1710 "Treatise on the Principles of Human Knowledge" (120) or "A Treatise concerning the Principles of Human Knowledge" (926)? Did Bonnet write an "Essai de psychologie" (128) or an "Essai de psychologie, ou considérations sur les opérations de l'âme" (927)? Did Darwin write a work entitled "The Origin of the Species" (929)? I had thought it was "On the Origin of Species by means of Natural Selection, or the Preservation of Favored Races in the Struggle for

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[1] There is, in the make-up of Pt. i, a blemish so obvious that one is astonished to find it escaping the notice of compiler, general editor, and printers. It is the absence of indication, in the headlines, of the particular philosopher under review or of the letter of the alphabet with which his name begins. The pages are headed, simply, "History of Philosophy"—"Philosophers." Suppose, then, that one is looking up, say, Cicero. One guesses at the place of the C's, and opens the book. Having opened it, one may have to turn over several pages before one finds the name of any philosopher. Having found some one, C or not-C, one has to turn back or forth until Cicero appears. Could anything be more awkward?

Life." I had thought, also, that the work here described as "The Descent of Man" (929) was published under the title "The Descent of Man, and Selection in Relation to Sex." Locke, so far as I know, did not publish "An Essay concerning the Human Understanding" (939). But, indeed, the compiler takes no thought of articles, whether they shall be retained or whether they shall be omitted. Baldwin is accredited with a "Mental Development in the Child and Race" (925: here the elision of an article has carried with it the omission of "Methods and Processes"). The work which Hume published in 1748 was called "Philosophical Essays concerning Human Undertanding," not "Inquiry concerning Human Understanding" (935: *cf.* 271). James, in 1892, published a "Text-book of Psychology," not a "Psychology" (936). Paulhan, knowing something of French, did not write a book entitled "Les intellectuelles types" (941), but rather "Les types intellectuels. Esprits logiques et esprits faux." And so on.

I pass to mistakes of another sort. Brentano's Psychologie (927) should have been given as Vol. I. Cousin's Elements of Psychology is said in one place (929) to have been translated by C. S. Henry in 1824; in another (159), to have been translated by C. G. Henry in 1834. Destutt de Tracy is misspelled as Tracey on p. 930. Here his Éléments is said to have appeared first in Italian, 1817-1819, and to have come out in French in 1825-1827; p. 180 tells us that the date of the first French edition is [1801]-1818, and that of the second 1824-5, while nothing is said of the Italian; p. 837 dates the two first (and French) editions 1817-18, 1824-25. On p. 927 van Biervliet's initials are given as J. I. instead of J. J. Elsenhars (931) should be Elsenhans. Ebbinghaus' Psychologie, i, is said (931) to have 194 pp. in the first edition; it has 694. Jodl's Psychologie of 1896 is said (936) to have appeared in two volumes. Lindner's Empirical Psychology was translated by C. de Garmo, not by A. J. de Garno (939). I did not, in 1891, speak of Professor Münsterberg as Dr. H. v. Münsterberg (941). Professor Karl Pearson figures on p. 942 as Pierson. Sully's Human Mind has two vols., not one (947); and the 501 pp. belong to the first vol. Dr. Aars is not named Aars-Kristian (950). Helmholtz' initials are H. L. F., not H. (955). Helmholtz did not write a Handbuch der Physiologie, Bd. iii, Thl. i, 1879; nor does the corresponding portion of Hermann's Handbuch say anything about hearing (1145). The Lipps who did *not* write a Grundthatsachen (but a Grundtatsachen) des Seelenlebens is also innocent of the Untersuchungen über die Grundlagen der Mathematik, ascribed to him on p. 1180. And so on, again.

I shall not, I hope, be accused of egotism if I allude to Dr. Rand's treatment of my own publications. If they are worth cataloguing at all, they are worth cataloguing accurately. The translation of Wundt's Vorlesungen is not mentioned on p. 535; in the Addenda et Corrigenda it is given under the date 1905. It appeared in 1894. On p. 963 I am said to have published a physiological vocabulary. On p. 914 my Primer is dated back from 1898 to 1892. I do not find that any translation of my books is mentioned, though (according to the Preface) "of the modern translations, English, German, French and Italian are regularly given, owing to their greater accessibility: other languages are, nevertheless, largely included." My initials are given, p. 963, as L. B. On the same page, I am reported as publishing, in 1901, an Experimental Psychology of 200 pp. In reality, I perpetrated in that year two volumes, with a much longer title, the one of 214, the other of 456 pp. "Dreams of Tasting" (1039) is not the title of a little paper which I published in 1895. My *Outline* did not reach a twelfth edition in the year after its first appearance (1136). Perhaps in order to compensate for all these sins of omission and commission, the compiler ascribes to me (1183) a work entitled The New Psychology, and published in 1897. I did not write it.

That a good deal of the above criticism refers to trivial points I shall not attempt to deny. What I affirm is that there are too many lapses of the sort specified. "In addition to the task of compilation," Dr. Rand informs us, "much labor has been expended upon the work of verification." And again: "During the past three years the author has given almost exclusive attention to the task of verification." Such attention ought, one would think, to have eliminated even printer's errors of the more glaring sort; it should at least have eliminated most of the bibliographical mistakes that I have pointed out. And these are but a tithe of the mistakes that I have noted.

There remains, behind all this criticism of detail, the fundamental question of the adequacy of Dr. Rand's work. Apart from these slips, is the bibliography comprehensive? may the student rely upon it? is it representative? I take the first group under *Bibliography G*, the group of psychological bibliography. Here the student of psychology is to find his working bibliographies. Well! we remark at once that the three yearly bibliographies of the Zeitschrift, the Psychological Review, and the Année psychologique are all duly mentioned. We notice, too, such obvious items as Dessoir's bibliography of hypnotism and the work of Ueberweg (spelled Ueberwegg). It is a little surprising, however, to find that the only personal bibliography is Wilson's list of the writings of G. S. Hall. Might we not expect a mention of the Fechner bibliography appended to Wundt's edition of the Psychophysik? Again: if it was worth while to mention the half-dozen pages on literature in Kuelpe's Grundriss (the pages quoted on p. 913, by the way, refer to the translation, and not to the original), would it not have been in place to cite the long chapter on the historical development of psychology in Villa's La psicologia contemporanea, 1899? Why should Foster's brief bibliography of sleep be cited, when no reference is made to the bibliographies of Manacéine, to which Foster acknowledges his indebtedness? The question is the more pertinent, since on p. 1037 Manacéine's bibliographies (there is the usual printer's error in the author's name) are specified in some detail. The Literaturverzeichniss in Jodl's Lehrbuch is mentioned; but the twenty-page bibliography by Spiller is not,—the Mind of Man appeared in 1902, not in 1903 (p. 946),—nor the twelve-page bibliography of Calkins' Introduction. The choice of text-books is curious. Jodl's Lehrbuch, Kuelpe's Grundriss, Münsterberg's Grundzüge, Sanford's Experimental Psychology, Sully's Human Mind, even my own Primer are quoted; but will it be believed that James' Principles and Wundt's Grundzüge are omitted? So with special bibliographies: we find references to taste and smell, to vision, to combinational tones (this last given as in the seventieth volume of the Philosophische Studien), and so forth; we do not find Bentley's or Kennedy's bibliography of memory, or Kinnaman's bibliography of lifted weights, or my own bibliography of the optical illusions. Nichols' Review of Recent Literature on the Perception of Time finds a place; not so the historical section of his earlier work on the Psychology of Time. Finally—to make an end—the bibliography of optics in Helmholtz' Handbuch should, of course, be credited to König.

The bibliographical section which I have thus passed under review occupies a page and a half of Dr. Rand's text. I shall not pursue the matter further. Enough has been said to show the character of the work, so far as concerns psychology. I sincerely hope that the parts of the two volumes which I have not examined are more nearly adequate and more carefully revised than those which I have read. Otherwise, I fear that Dr. Rand will have labored in vain,— unless, indeed, he is content to have his pages serve simply as rough copy for future bibliographers. E. B. T.

L'Ame et le Corps, par ALFRED BINET. Bibliothèque de philosophie scientifique. Ernest Flammarion, Paris, 1905. pp. 288.

The contents of this work are, in sum, the following:

Book I, *Definition of Matter*. Of the material world we know nothing but our sensations. As all sensations are alike mediated by the nervous system, it is impossible to make a distinction and regard certain of them only as objective, *e. g.*, those of touch, vision and the muscular sense, while regarding others as merely subjective. The mechanical theory, therefore, which finds a specially objective significance in movement, has only the value of a symbol. Elsewhere Binet characterizes it as a species of fetchism (p. 227).

Book II, *Definition of Mind*. Instead, however, of drawing the 'idealistic' conclusion that all material phenomena are states of consciousness, Binet draws the opposite conclusion, namely, that the contents of all sensations are physical, *i. e.*, material, phenomena. From the content he distinguishes, as the mental side of the complex fact, the act of cognition. The same analysis applies to images and ideas (conceptions) which are derived from sensation, and it would also apply to emotions, desires, appetites, etc., but for the suspicion that the theory which reduces all these to sensations (objects) is too simple and artificial. The fundamental relation is not that of subject-object, for the subject is really an object; the subject term is replaced by the 'act of cognition.' The relation of cognition to objects is purely contemplative, it is only a consciousness; nothing is added by the categories of the understanding; relations cognized, resemblance, for instance, are physical properties. While consciousness is inseparable from objects, objects may continue to exist without consciousness; the unconscious is merely the unknown which can or could be known under certain conditions. The only bond capable of connecting different mental states is the material. As sensations and images or ideas are material elements, psychology, paradoxical as it may seem, is a science of matter, the science, namely, of a portion of matter having the property of preadaptation (p. 181).

Book III. *The Union of Soul and Body*. The problem of the union of mind and matter is not the problem of two heterogeneous things, the direct relation between which is incomprehensible. The relation is rather that of form (consciousness) and matter (content, sensation, object), as Aristotle taught. The difficulty is not to unite, but to separate them. The separation is found in the incompleteness of the life of consciousness; consciousness requires matter as its correlate, whereas matter does not so require consciousness. The genesis of consciousness cannot be explained; it has equal rights with matter. These principles serve for the criticism of the current theories of spiritualism and idealism, materialism and parallelism. The hypothesis of the relation of soul and body has to satisfy, in particular, two conditions: (1) the manifestations of consciousness are determined by processes in the brain, and (2) of these its immediate conditions consciousness is absolutely ignorant. Binet's hypothesis is as follows: The effect (neural wave) contains, or has inscribed on it, or is the depositary of, the totality of the physical properties perceived in the cause (object). This is an 'absolute certainty.' But the undulation contains, besides the qualities it receives, qualities it itself contributes. To these last, however, as the relatively constant factors, consciousness, according to a fundamental law of its nature, remains insensible; it only perceives the variable and accidental properties which express the nature of the stimulus. The result is thus equivalent to a transformation; it is not, however, the transformation of a physical into a mental phenomenon, and properly speaking there is no transformation, but only analysis.

The fundamental error in this whole argumentation, in the writer's opinion, is the utterly uncritical identification of sensation, sensory content, perceived qualities and material object. If sensations are material phenomena, then no doubt images, ideas and conceptions are material phenomena too, and the author is quite right in his paradoxical contention that psychology is a science of matter. But the very paradox of this conclusion makes one suspicious of the premises. Mind, on M. Binet's assumptions, seems to me to be left without form or content and the material world to be void of any principle of permanence or continuity. That sensations can exist without consciousness is a pure assertion. Granting, even, that 'sensation' is to be taken as meaning 'perceived quality,' are we really to suppose that the multitudinous shapes and sizes of things, which vary with every change in the conditions of their perception, continue unchanged as material properties independent of these conditions? If not all, why some, or any? Subjective idealism, absurd as it may be, is surely a saner and more natural conclusion than this bizarre realism. I cannot but think that a more thorough analysis of what we mean by objective experience, or the reference of objects to consciousness in general, would have led to the discovery of factors undreamed of in M. Binet's philosophy. But to this meaning his abstractedly objective conception of the categories has fatally blocked the way. But as he has not adequately analyzed experience, so he has not given, as it seems to me, either a satisfactory definition of matter or a satisfactory definition of mind, and consequently he has not solved or even properly stated the problem. Incidentally the contradictions in his thought appear, as when, speaking of the catagories, he denies that the mind creates relations and declares intelligence to be but an inactive consciousness (p. 121); yet he constantly speaks of 'acts' of consciousness and explicitly affirms of reasoning that it is an activity which creates relations (p. 176). Still more striking is the confusion when in the very same context he states, first, that all our knowledge of the properties of matter is by (*par*) sensation (p. 65) and then (p. 66) that all we know of matter is not known in or by (*par*) sensation, but is the sensation itself.

Smith College. H. N. GARDINER.

Paedagogische Psychologie, von L. HABRICH. Kempten, 1903. pp. lxxii, 660.

This work—issued in 1903, but only recently received by the *Journal*—is an applied psychology written for use in Roman Catholic schools: it contains "die wichtigsten Kapitel der Seelenlehre, unter durchgängiger Anwendung auf Unterricht und Erziehung vom Standpunkte christlicher Philosophie anschaulich dargestellt." Of its two parts, Pt. i, Das Erkenntnisvermögen, had reached its second edition with a sale of 8,000 copies within a year from the date of publication; a sufficient evidence of the suitability of the book to the purpose for which it was intended. Pt. ii, Das Strebevermögen, appears here in its first edition.

The plan of the work is that of a psychology in duplicate; the theoretical sections are followed, under each heading, by sections on application. The present reviewer cannot approve this plan, but agrees rather with the "oberste Schulbehörde in Preussen" that theory should come first, as a whole, and application be made later in a separate course. However, this is a matter of opinion only, and the author writes from ripe pedagogical experience. The work itself is a highly articulated system, couched in the spirit and largely in the language of Aristotelian scholasticism, and leaning heavily on the larger books of Pesch and Willmann. So far as its psychology goes, it is a product of the desk, and there is no evidence that the writer has thought—still less experimented—in his own person and at first hand. Occasional references are made to the leading works upon scientific psychology; but rather, it would seem, with the view of showing that Catholicism need not fear the new movement than through any sympathy with, or real understanding of, the modern trend. The student will, therefore, gain from the work very little knowledge of the present status of psychology, though he will be mightily reassured as regards the immortality of his soul. "Was gelten mir alle Lehren von der Wahrnehmung," exclaims the author, "der Vorstellung, dem Gedächtnis, oder die neueren Fragen von der Ermüdung; von der physiologischen Zeit, oder die Fragen der Kinderpsychologie: von der Entwicklung des Sehens, des Hörens, des Sprechens, was gelten alle diese Fragen, so anziehend sie sein mögen, neben der Hauptfrage: Habe ich eine unsterbliche Seele? Sie sind ein unbedeutender Kleinkram neben der grossen Hauptsache."

M. W. WISEMAN.

Anatomie du système nerveux de l'homme: Lécons professées à l'Université de Louvain, par A. VAN GEHUCHTEN. 4me. edition. Louvain, 1906. pp. xv, 999.

We need do no more than call attention to the appearance of a fourth and revised edition of this excellent treatise. The first edition was published as recently as 1893; and the fact that a fourth edition has become necessary in the course of thirteen years is a sufficient guarantee of the usefulness and authoritative character of the work. As regards the thoroughness of the revision, we may quote a sentence from the new preface. "Nous n'étonnerons certes personne en avouant, en toute sincerité, que ce travail considérable de révision a exigé de notre part un labeur de tous les jours poursuivi pendant près de deux ans." The result is worthy of the author and of the science which he represents.

In 1900, contemporaneously with the publication of a third edition of the present work, the author began the publication of his journal "Le Névraxe." The papers which have appeared in this journal have, of course, been largely drawn upon in the preparation of the new edition. Regard is paid to the new light thrown upon the old questions of the reticulated structure of cellular protoplasm, nervous regeneration, the unicellular or pluricellular origin of the neurone, etc., etc. In the discussion of the paths of nervous conduction, the arrangement of former editions, based on Flechsig's demarcation of the cortex, has been given up. The author now considers, separately, first the ascending paths that carry sensible impressions to the cortex, whatever their nature, and whatever the organ, internal or external, sensory or non-sensory, destined to receive them; and secondly the descending paths, cortical or sub-cortical, mesencephalic, pontal, medullary or spinal, by means of which the nervous system is enabled to respond, consciously or unconsciously, to the stimuli which play upon it. This mode of treatment renders it possible to consider the long and the short paths by themselves, and to bring the reflex paths into a natural connection with the latter.

In view of the recent controversies regarding the neurone theory, the author's conclusion may be interesting. He writes as follows

(p. 196). "Vous voyez, par l'examen des différentes théories, que la doctrine de l'indépendance anatomique des neurones reste debout malgré l'assaut qu'elle a eu à subir de divers côtés et malgré les tentatives nombreuses, mais infructueuses, faites par un certain nombre d'autres, pour arriver à établir l'existence réelle d'une continuité anatomique entre tous les éléments entrant dans la constitution de la substance grise des centres nerveux." P. E. WINTER.

The Works of Lucian of Samosata: complete with exceptions specified in the preface. Translated by H. W. Fowler and F. G. Fowler. 4 vols. The Clarendon Press, Oxford, 1905. pp. xxxviii, 248; 275; 280; 247.

These volumes open with an Introduction, in which the translators discuss what is known of the life of Lucian; the probable order of his works, the circumstances of his time, and his position in the world of letters. Then follow translations of all the recognized works, with the exception of seven of the Dialogues of the Gods, one of the Dialogues of the Sea-gods, two chapters of the True History, two chapters of the Alexander, the Eunuch, a chapter of the Rhetorician, a chapter of the Book-fancier, three of the Dialogues of the Hetaerae, and the Pseudologista, De Syria Dea, Tragodopodagra, Ocypus and Epigrammata. The fourth volume concludes with a batch of Notes explanatory of Allusions to Persons, etc., and each volume has an alphabetical table of contents of the whole work.

It must be said at once and without reservation that the translation has been very well done. Theoretically, Lucian should be untranslatable, just as those other three, Rabelais and Voltaire and Heine, should be untranslatable. But the instance of Rabelais in particular shows what may be done in English with a genius of Lucian's type. And the translators have, in fact, succeeded in writing Lucian in English, so that he who runs with humor, however innocent of Greek, may read interestedly and intelligently, though of course, here as everywhere else, a knowledge of Greek brings advantage that cannot be overestimated. Nor can the present reviewer find any difference of quality—the difference that his critical instinct prompted him to look for—between the translations of 'F.' and 'H.' To both translators the work has evidently been a labor of love, and love was competent to its task.

There is, to be sure, the vexed question of expurgation and omission. Expurgation by a translator is something of an impertinence both to author and to reader; while, on his side, the translator may assert the right to lay down his pen when and where it pleases him to do so. It is regrettable that we cannot have our Lucian complete, because, unless complete, we have not Lucian. Is it regard for the enquiring schoolboy, or for Mrs. Grundy at large, or is it a publisher's scruple, that has borne upon Dr. Merry and the translators? Not, surely, any puritanic reservation in their own minds; for no puritan could so have caught the spirit of the first cosmopolitan.

It is a minor flaw that the Notes are collected at the end of the last volume. Most of them take up only a couple of lines, and none exceed half a page of fine print. They could, therefore, easily have been printed as footnotes; the instructed reader might easily have passed them over; and the uninstructed would have been spared the trouble of keeping two volumes going at once. Besides, the translators are not consistent in the use of their own principles (iv, 191). Why should we be told in a footnote that "*Ctesis* is Greek for 'gain,'" while we are referred to the end of the last volume for a note, say, on Adonis?

This question of the Notes suggests a final possibility. Might not the attempt be made to modernize the names, at least in certain of the works? Mr. Andrew Lang has told us that Peregrinus cremated himself on the Epsom Downs, just after the Derby. Is not the hint worth taking? Modern as he is, Lucian must lose something of his modernity so long as the Greek names are kept. Kept, sometimes, they surely must be; but not always. And, in a second edition, let a bracketed word be said to distinguish the sex of Lucian's *personæ*. When the Greekless reader sees "Joessa, Pythias, Lysias," how is he to know that Pythias is a girl and Lysias a man?

L. E. STRANGEWAYS.

In Quest of Light, by GOLDWIN SMITH. The Macmillan Co., New York, 1906. pp. viii, 177.

Individuality and Immortality; the Ingersoll Lecture, by WILHELM OSTWALD. Houghton, Mifflin & Co., Boston and New York, 1906. pp. 74.

In the first of these two little volumes, Mr. Goldwin Smith republishes some five and thirty brief communications made, chiefly in the form of letters, to the New York Sun during the last decade. The writer's position may best be stated in his own words. "Dogmatic and miraculous Christianity we resign. But the vital principles of Christianity, the fatherhood of God and the brotherhood of man, still rest on their historical and moral evidences as a key to the moral problem of our being" (122). "Our only hope of salvation lies in the full and hearty, though reverent and discriminating, acceptance of that which is now the revealed truth, though reason is the organ of the revelation" (p. 56). "I heartily accept evolution, only pausing to see whether a discovery so recent as well as momentous has yet found its final level. I only ask that certain phenomena of human nature, its liberty of choice in action, its moral aspirations, its power of idealization, its finer affections, its sense of spiritual beauty, . . . all in fact that constitutes what we have regarded as spiritual life, should receive fair consideration, and that we should be told whether these phenomena can be explained by evolution or by any process of material development" (p. 85). "Such fancies as spiritualism, telepathy, planchette, seem to be the offspring of a . . . void in the soul, created by the departure of traditional religion. They will not help us to save or revive our spiritual life. They will . . . seduce us into grovelling superstition" (p. 106).

Professor Ostwald raises the question, What has Energetics to say about immortality? He starts out with memory, in Hering's sense, as a universal function of living matter. From it he derives the evolution of organisms into classes and species, as well as the facts of heredity. Here he digresses to oppose Wiseman's idea of the immortality of the protozoa. Returning to memory, he explains by it the functions of mind, and especially the belief in the objective existence of a real world. So he passes to the scientific conceptions of mass and energy, which have, if anything in science has, a right to be called immortal. But he points out that "all of our inferences about eternity are based on extrapolation from finite time and observations coupled with a certain error." This is illustrated by the fate of the doctrine of conservation of the elements. Then, leaving this line of thought, he emphasizes the tendency of mass and energy towards diffusion. As with inorganic nature, so with man: increase of culture tends to reduce individual differences, and the happiest moments of our lives are impersonal. Not even a collective being, however, is immortal: only longer lived than the individual. As for man himself, his sole hope of immortality is to leave certain things in the world, after his death, changed by his influence; and such a prolongation of individuality is not immortality in its strictest sense. It is, however, "the only lasting kind of life that I can discover in the realm of our experience." Ethics is rather purified and strengthened than threatened by this conception. P. E. WINTER.

Beiträge zur Psychologie und Philosophie, herausgeben von GOETZ MARTIUS. Erster Band, 4. Heft. Leipzig, 1905.

(1). G. Martius, Ueber die Lehre von der Beeinflussung des Pulses und der Atmung durch psychische Reize.

(2). C. Minnemann, Atmung und Puls bei actuellen Affekten.

Dr. Martius' paper attempts to arrive at an understanding of the discrepant results which, up to the present time, have followed the application of the method of expression to the study of the feelings. Several serious faults in technique are brought out; and, with a plethysmograph so constructed as to avoid these errors, a study of the simpler affective processes is made by Martius, and of the emotions by Minnemann. After alluding to the wide differences in result, as regards the question of the influence of mental processes on pulse and respiration, Martius points out that these differences must mean either that the method is inadequate and the problem wrongly formulated, or that the method has not been applied with the proper precautions. The latter possibility is the more probable for two reasons: (i) faults in technique; (ii) errors in interpretation. Three faults of technique are mentioned: (a) the effect of respiration on the rate and height of pulse; Martius is convinced that expiration heightens and slackens pulse; (b) the methods of measuring the rate of pulse. Two methods are aimed at in this criticism: the determination of the number of pulses for some arbitrary period (usually five or ten seconds), and the method used by Lehmann, of dividing the curve into variable periods, apparently according to the *niveau*. The criticism is the same for both methods: that the influence of respiration (the slackening and heightening mentioned above) vitiates the results. (c) The lowering of the height of the volume pulse by the fall in volume of the arm. This fact has been explained on the supposition that fall in volume caused a reduction of the blood pressure; the height of pulse would, if that were true, be less. Martius, however, finds that the mere drawing out of the arm from the plethysmograph brings about a fall in volume *with a reduced height of pulse*. He thinks that this reduction is due to the rarefaction of the air in the manometer and the consequent poor transmission of the impulse. The chief error of interpretation regards the significance of the longer waves that are to be found in nearly all plethysmographic records. Briefly, Martius' view is that all changes in level, except the respiratory oscillation, are due to unconscious movements of the hand or arm or, perhaps, other parts of the body. In this belief, he constructed a plethysmograph such that all movements of the body were rendered ineffective. The instrument consisted of a metal sleeve which was provided with a manometer and a cock for admitting water. The sleeve was slipped over the fore-arm. The joint of the metal and the arm was made with plaster of Paris and, further, the arm thus encased in the sleeve was securely attached by the same means to a board which was clamped to the table. In this way, there could be no pushing or pulling of the arm in or out of the plethysmograph. The records were obtained in the usual manner. The measurements of the curves are the lengths of each individual pulse or respiration. Preliminary records with this instrument show that (i) in proportion as the movements and the possibilities of movement are excluded, the smaller and less important do the volume changes become; and that (ii) of the changes that remain, the irregular sinkings and rises disappear, while the regular changes persist (*cf.* p. 449). Martius finds that the curve taken under the conditions described is very similar to the curve recorded by the Lehmann plethysmograph with the cock open. This fact is, probably, the justification for Minneman's practice of using the Lehmann plethysmograph with the cock open.

The following topics were investigated:

1. Influence of artificial changes of respiration on pulse.
2. " " bodily work (ergograph).
3. " " mental activity (arithmetic).
4. " " bodily pain.
5. " " smell and taste.
6. " " artificially produced emotions.

The results were:

1. Acceleration of breathing causes accelerated pulse. Deepened breathing accelerates pulse. Slower breathing slows pulse.

2. Rate of both pulse and breathing is increased. The height does not change consistently; it tends, perhaps, to be less.

3. Rate and height of both pulse and respiration are increased.

4. Mixed. Rate of pulse is, in most cases, faster; height, where changed, less. Respiratory changes are equivocal.

5. As regards pleasantness and unpleasantness, there are no constant characteristics.

6. There is an emotive type, characterized by slowing of pulse and respiration, which is the opposite of an active type.

The last result reminds one of the view advanced by Binet, that the opposition of feelings and emotions is not between their pleasantness and unpleasantness, but between activity and repose.

The occasion of Minnemann's work was the possibility that the reproduced emotions, which were the object of Martius' study, might have different expressions from naturally aroused emotions. The emotions of mirth, joy and hope were set up by suitable communications. Deception, expectation, sympathy, fright, anger, excitement were also aroused. Five persons took part in the experiments. The technique of the investigation was, practically, the same as that of Martius. Both pulse and respiration were recorded and the changes in rate and height measured. The average rates and lengths of pulse and respiration, for each observer, for the normal and the reaction periods, were measured and platted. The essential result of this work is that there are no qualitative differences of emotions which, at least by the method and under the conditions of the author, find expression in different bodily changes; the opposition is rather between the exciting and the depressing emotions. The Kantian classification of sthenic and asthenic emotive states seems to receive from this study experimental confirmation.

To the present reviewer, the most important result of Martius' work is the apparent demonstration that the vaso-motor waves are not of normal occurrence in human adults. If we grant the fact, it would seem to throw doubt on the existence, other than as artifacts, of the waves in animals under experimental conditions. A second result, which adds itself to a gradually increasing mass of evidence, is the failure to find characteristic symptoms for pleasantness and unpleasantness. What one does not find, although there is some reason to expect it, is any evidence tending to show that the reactions of pulse and respiration depend upon the quality of the stimulus used. Aside from the first two points mentioned, the critical tone of the work will, doubtless, be its most effective result.

University of Washington. H. C. STEVENS.

Future Life in the Light of Ancient Wisdom and Modern Science, by LOUIS ELBÉ. A. C. McClurg Co., Chicago, 1906. pp. xxviii, 382.

The object of this book is to present, in popular form, the evidence for human immortality. It is divided into two parts, the first appealing to the *consensus gentium*, the second to the results of modern science. In the former, the author passes in rapid review the testimony of prehistoric traditions and remains, the beliefs of savages, and the ideas of life after death entertained by Chinese, Egyptians, Hindus, Chaldeans, Gauls, Jews, Greeks and Romans. The notion of immortality "sums up the whole teaching of ancient wisdom." There follows a chapter on Christianity, with discussions of the Roman Catholic doctrine of purgatory and the Protestant conception of conditional immortality; and the part ends with a review of two modern systems, whose main tenets are drawn from antiquity,—spiritism and theosophy. The first of these makes man "a fallen god who remembers;" the second, a future god who is attempting to climb to heaven.

Passing to science, the author points out that astronomy suggests to us the possibility of a plurality of worlds and, by banishing a material heaven and hell, transfers the scene of our final destinies to an immaterial plane. Physics gives us the law of the indestructibility of matter and energy, which includes the indestructibility of past events, including the events of our consciousness. As physics appeals to the ether as the source of the most diverse manifestations of energy, so we may appeal to etheric radiations or an astral envelope as forming the necessary link between the immaterial soul and the physical body of man. True, "we are always confronted with a fundamental difficulty in endeavoring to prove the distinct existence" of an odic fluid; it still remains to be proved "that the deviations of the biometric needle are amenable to no other explanation;" the authenticity of photographs "is still a matter of dispute;" and experiments with phosphorescing calcium sulphide do no more—alas! poor Odic!—than identify the radiations with the *n*-rays of Blondlot. Still, when we take into account their externalization in the ethereal double, and the facts of long-distance telepathy, we are forced to admit that "the etheric movements by which we are wont to explain the action of the physical forces are not possessed of more certain reality;" and what carries conviction in the one case should do so in the other. Thus formulated, faith in survival seems "to be the inevitable consequence of the scientific conception of the human soul."

M. Elbé writes brightly, and his book is readable. He has a good deal of critical acumen, and a distinct power of marshalling arguments. But this book will, of course, convince those and those only who are already prepared to accept its conclusion.

H. E. HOTCHKISS.

Sociological Papers, II. By F. GALTON, P. GEDDES, M. E. SADLER, E. WESTERMARCK, H. HOEFFDING, J. H. BRIDGES and J. S. STUART-GLENNIE. Macmillan & Co., London, 1906. pp. xiii, 312.

The eleven original papers published in this volume fully maintain the high level of work reached in the previous publication of the Sociological Society. We here find sociology approached by many paths and envisaged in various ways; we find the widest divergence of opinion; but we also find an earnest spirit of co-operation, and a refreshing amount of solid thinking.

The historical approach to sociology is represented by Dr. Bridges' paper on "Some Guiding Principles in the Philosophy of History," and by three articles from the pen of Mr. Stuart-Glennie, entitled respectively "The Place of the Social Sciences in a Classification of Knowledges," "The General Historical Laws, the Anthropological Bases of a Science of Socialization," and "The Application of General Historical Laws to Contemporary Events." The ethical approach finds its representatives in Professor Hoeffding, who writes on the Relation between Sociology and Ethics, and in Dr. Westermarck, who contributes an essay on the Influence of Magic on Social Relationships. An attempt to apply psychology to sociology is made in Professor Sadler's article, "The School in some of its Relations to Social Organization and to National Life." The biological course is pursued in three very important papers by Mr. Galton, all dealing with his own science of eugenics: "Restrictions in Marriage," "Studies in National Eugenics," and "Eugenics as a Factor in Religion." Finally, the geographical approach is represented by the second part of Professor Geddes' memoir on "Civics as Applied Sociology," the first installment of which appeared in the preceding volume. Not the least interesting part of the contents of the present collection is the Discussion—formal and informal, written and spoken—appended to the original papers. M. W. WISEMAN.

The Color Sensitivity of the Peripheral Retina, by JOHN WALLACE BAIRD. Published by the Carnegie Institution, Washington, May, 1905. pp. 80.

The first thirty-four pages of this paper are devoted to an exhaustive review of the work already done on peripheral vision. This review is exceedingly valuable, as it makes clear the present status of the problem.

Dr. Baird finds much greater uniformity of opinion with regard to the change observed in the tone of each color as its image is moved from the fovea to the periphery of the retina, than concerning the relative extension of the color fields, since the very decided differences in the methods employed by various investigators would naturally prove much more productive of disagreement in a strictly quantitative measurement than in the mere observations of a qualitative change.

Many investigators have failed to equate in brightness and intensity, the colors they have used. One field, for example, that of red, has been determined with an intense stimulus, and another field, in the same experiment, perhaps that for green, with a stimulus of less intensity. Under these circumstances, there seems to be no reason why any co-extension of fields should be discovered even though it really existed, since it is a well known fact that intensity of stimulus directly affects the visibility of a given color.

The following is a summary of the results so far agreed upon.

"It has been established that color sensitivity decreases gradually from the centre to the periphery of the retina; that every color stimulus is correctly recognized within a certain retinal zone, whose extent varies directly with the tone, the brightness (absolute and relative), the saturation, and the area of the stimulus, and with changing conditions of adaptation and of refraction; that under certain conditions[1] the zone of a certain red[2] is co-extensive with that of a green, while that of yellow is also co-extensive with that of blue; that the yellow-blue zone has a much wider extension than the red-green zone; that all colors, excepting the four mentioned above, pass through certain regular transitions of tone as they appear upon more and more peripheral regions of the retina; that these transitions tend in the direction of yellow (when red, orange or green stimuli are employed) and blue (when violet stimuli are employed); and that with moderate stimulation all colors appear gray at the priphery, while with a sufficiently intensive stimulation they may appear in their own tones."

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[1] By "certain conditions"—the author means that external conditions remain constant, and the colors are equated in brightness and saturations.

[2] "A certain red"—*i. e.*, a stable red or one that undergoes no change in tone as its image is moved from the forea to the periphery. All four colors referred to are stable colors.

The experimental work reported in this paper was carried out in the Psychological Laboratory of Cornell University, four Cornell students as well as Dr. Baird serving as subjects.

The apparatus was a duplicate of the Hellpach perimeter, which consisted of a steel quadrant of 1.1 meter radius, supported at a convenient height. The stimulus lantern, which could be clamped at any point desired on the quadrant, was enclosed in a black box having a circular opening 15 m. m. in diameter in the side toward the observer. This opening was covered by colored gelatin sheets combined so as to give the desired colors. The color was in each case subjected to spectral analysis. The fixation point consisted of a spot of light 5 m. m. in diameter.

The tests were all made in the dark room, the eyes being thoroughly dark-adapted. The stimuli were stationary, of three seconds duration and separated by an interval of six minutes.

Part I of the experiment is concerned with the determination "of the changes of tone which a color-stimulus undergoes during the movement of its image across the retina."

Seven spectral colors were employed as stimuli, *i. e.*, deep red, reddish orange, orange yellow, green, blue, violet and purple. As no comparison of the color fields was intended the colors were not equated in brightness and intensity. Three intensities of stimuli were used. The colors employed by Hellpach could not be exactly reproduced, as, owing to inaccuracy in his statements, it seemed impossible "to discover what were the wave-lengths of the stimuli employed in his experiments."

The results of part I are as follows: (1) "With a slight intensity of stimulus all colors appear colorless at the extreme periphery of the retina," (2) "When they are brought in far enough to appear colored, those of the red end of the spectrum first appear yellowish or yellow, while those of the blue end first appear bluish or blue; and that in assuming their proper tones they pass through a regular series of transitions." Stable red and green would be the only exceptions to the above statement. (3) During any single stimulation the color, in fading out, goes through a series of transitions in brightness and color-tone, analogous to those experienced when the image of the color is made to move from a less pheripheral to a more peripheral region of the retina.

4. As the stimulus is made more intense the field for a given color is increased in extent.

5. *After-images* in the ordinary sense, were reported in but a few cases, and then only when the paracentral regions of the retina were stimulated. Though Dr. Baird failed to find after-images when peripheral regions of the retina were stimulated, he insists on the presence, in that region of certain *after-effects;* wholly latent in character but nevertheless, at times, influential in determining the color-effect of succeeding color-stimuli. He explains Hellpach's 'Gegenfarbige' zone (*i. e.*, an extreme peripheral zone in which the colors appeared in their complimentary instead of their true color tone) as due to these after-effects.[1] Our author found that results similar to those of Hellpach could be produced at any point on the retina by making the interval between stimulations short enough but that the compliment-

ary tone would disappear altogether provided the intervals were long enough. For example, yellow stimuli, given in succession with short intervals between stimulations, could be made to appear as blue, but if a long interval were interposed it would again appear as yellow. The following example will serve as an illustration.

Stimulus—*Yellow;* interval, *one minute.*
appeared at 65° as Yellowish.
" at 55° as Yellow.
" at 45° as Blue.
" at 35° as Blue.
After a *Rest of five minutes in darkness*
appeared at 35° as Yellow.
" at 45° as "

Dr. Baird gives no results of experimentation in which an interval equal to that allowed by Hellpach (*i. e.* three minutes) was introduced. The statement is made, however, that many tests were carried out with varying intervals, before the six minute interval was finally decided upon.

It seems surprising that such decided after-effects did not appear in the form of after-images and the question still remains whether they would not have done so had the stimulus been more intense and of longer duration.

It is also to be noted that these conclusions concerning the absence of after-images on the peripheral retina have not been established in the case of the light-adapted eye.

The second problem investigated by Dr. Baird is that of the relative extent of the color fields. In this part of the work the four stable colors were used exclusively as stimuli, *i. e.*, the yellow, the blue, the green and the red which undergo no change in peripheral vision.[2] It might be stated here that only four colors were found which do not change in tone as they are moved from the fovea to the periphery of the retina.

"The results show that the zone of a stable red is co-extensive with that of a stable green; that the zone of a stable yellow is co-extensive with that of a stable blue; that the yellow-blue zone is much more widely extended in all directions than is the red-green zone; that the nasal side of the retina has the widest extension of color sensitivity, and that there is a wide individual variation in zonal extension."

These results are exceedingly interesting as this is the first dark-room experiment in which the four color fields have been determined with colors of equal white and color-values. The result is the discovery that the coincidence of the blue and yellow fields, and of the red and green fields, already established for the light-adapted eye; also holds for the dark-adapted eye.

The results agree with the Hering and the Ladd-Franklin theories of color vision but cannot be reconciled with any other theory.

Bryn Mawr College. GRACE M. FERNALD.

Les Mystiques—Etude psychologique et sociale, par PAUL HERMANT. Rev. de Synthes. Historique, 1905, Vol. 10, pp. 269-292; Vol. 11, pp. 32-53, 165-180.

This long study abounds in interesting quotations but is too indeterminate to have any definite historical, philosophical or psychological significance. The more valuable feature of Hermant's work is to be found in the parallels which he draws between Sufi, Buddhist, Moslem

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[1] Hellpach's intervals between tests were of three minutes duration.

[2] Dr. Baird has called my attention to a slip on p. 60, namely, the statement that "the red stimulus transmitted no part of the visible spectrum." What is meant is evidently that the red stimulus is non-spectral.

and Christian Mysticism, with the result that the essential characteristics of Christian Mysticism are seen to be common to all the religions considered.

JAMES H. LEUBA.

Practical Dietetics, with Reference to Diet in Disease, by ALIDA FRANCES PATTEE. A. F. Pattee, Publisher, New York. pp. 300. Price, $1.

Any home-maker will find this book valuable. It gives explicit directions and plain reasons. An inexperienced person, able to follow such directions, may prepare successfully and serve properly food for the sick or for those needing to be careful in diet. In a few pages and without wearisome detail food values are set forth. Rules follow for feeding the sick and for serving their food daintily. Over half the book consists in exact recipes for food and drink, with precise directions from the first step to the placing before the patient. One is not left to "season to taste." Diet in Disease takes sixty pages, Diet in Infancy, fifteen; "Practical Suggestions to the Nurse in the Sick-room" is especially useful to the novice; tables of measures, and two complete indexes make the last of the three hundred pages. The head of the household will find this volume an excellent supplement to her professional library.

FLORENCE B. SANFORD.

BOOK NOTES.

Die Stofflichen Grundlagen der Vererbung im organischen Reich, von EDUARD STRASBURGER. Gustav Fischer, Jena, 1905. pp. 68.

In very many species the ova from which male and female arise is easily distinguishable by size, and this has favored the idea that sex is already determined in the egg. On the other hand, unfertilized eggs of bees produce males, that is, the so-called drones, while fertilized eggs produce females. Here, then, it would seem that fertilization determines the female sex. Some, however, hold, despite this, that bee eggs are male and female and that is only the female eggs are adapted to fertilization. Again the Hoffacker-Sadler law that male offspring predominate if the father is older than the mother and more girls are born if the mother is the older, and that the prospect for boys is but slightly greater than for girls if the father and mother are of the same age. So many objections have been raised against this law that it is at present uncertain. Still we cannot say that the exclusive influence of the female in determining sex is, at present, entirely disproven for the human race. For horses, Wilkens states, on the ground of copious statistics, that only the age of the mare affects sex, and that mares, when they are becoming older, tend to produce more stallions, no matter what the age of the male horse may be. Thus Strasburger thinks that, as in so many other cases, there may have been a division of labor between the male and female determination of sex, and that by giving it over to the egg a constant numerical relation of the sexes is best assured.

What advantage does an organism derive from the exchange of pangenes which apparently takes place in the gonotokonts within the fused ids and the effect of which is further increased by the division of the chromosomes? Weismann concluded that the products of the two sexes differed from each other in their content of the material of heredity. By the amphimixis of these products the visible individual differences of posterity arise. They, too, make possible the permanent elimination of combinations of less value by means of natural selection, and thus keep the species at the top of its adaptation. At the same time natural selection creates material for the development of new species. Against this view, the objection is raised that fertilization causes only changing combinations of marks already present, but cannot create really new *Anlagen*, so that the origin of these must be sought elsewhere. Strasburger, however, agrees with Weismann that amphimixis favors the continuance of a species. For him the chief value of fertilization lies in the *Ausgleich* which the individual variations mutually experience. This *Austauch* of the pangenes in the prophase of the reduction division brings advantages for the species and hinder extreme development of deviations. Thus the offspring of the same pair of parents can never be quite alike. The manifoldness of individual variations increases the effectiveness of a species in competition with other species. Amphimixis, then, makes the resulting organism more plastic within the field of its variations, and this equips it for its work in life.

Sex and Character, by OTTO WEININGER. Authorized translation from the 6th German edition. G. P. Putnam's Sons, New York, 1906. pp. 356.

This is a really extraordinary as well as a much-advertised book. It is a product of precocious genius. Its author committed suicide in Beethoven's house when he was twenty-three years of age because he was too deeply involved in the pessimism of Schopenhauer. The cardinal position of the author is that what Haeckel calls gonochorism or the separation of the sexes, the sharpest form of which is in sexual dimorphism, is a matter of degrees. Most people are more or less bisexual and their differentiation is incomplete. Sexual attraction, our author holds, always tends to be mutual in the end. It is a discredit to the race that we have so little knowledge of the laws of sexual affinity. The best offspring will result from unions where there is a maximum of sexual suitability, such as proverbially occurs in love children. To judge this from a moral point of view is absurd, but it is important to know that the offspring of those parents which had greatest sexual attraction succeed best. There is no true psycho-sexual hermaphroditism. The men who are attracted by other men have marks of effeminacy, and just so women who attract other women sexually are more or less masculine. Perhaps there is no friendship between men or between women without some element of sexuality in it so that homo-sexual phenomena belong to intermediate forms. One who is half man and half woman requires a complement of the same dual kind. "The more feminine a woman is the less she will understand a man and the greater influence will he have upon her." The craving to acquire a man's character, freedom, interests, creative power, as do the emancipated women, is due to the fact that they are imperfectly sexed. They are homo-sexually or bi-sexually inclined. Much so-called intellectual activity of women is hysteria. "Even the malest woman is scarcely more than fifty per cent. male." So it is absurd to prate of full equality. An age dominated by the woman's movement would mean intermediate sexuality or imperfct cleavage. The author agrees with Nietzsche and Kant that from women nothing can be learned of women. They lack self-knowledge because they have no duality, whereas man is conscious of his sex because he has other elements in his nature and can act against it. Weininger proposes the term "hen id" for those who cannot distinguish between perception and sensation, and who want to say things and find them gone. Woman, he says, has an unconscious life, man a conscious life, and genius the most conscious life. Genius is nothing but the full completion of an idea of a man, and therefore every one should have some of it. It is simply the highest morality and is thus every one's duty. It is identical with universal responsibility and until that is grasped it will only be a wish and not a determination. Woman is wanting in super-sexual personality. "The absolute female has no ego." "The man of genius possesses, like everything else, the complete female in himself," but woman is a part only and not the whole, and therefore can never include genius. She is not a monad and can never reflect the universe. When she "understands" a man she simply tastes what he has thought about her. She can converse and chatter, but can never talk. Women have no dignity. The word "lady" was invented to fill a vacuum. Women have no soul. They are non-moral. There are two types: one is the coquette, who is always at heart a courtesan, and at the opposite pole stands the true mother. The first thinks only of the man, the second only of the child. Perhaps neither absolute type has any concrete existence. The motherly girl is more or less motherly even toward the man she loves. That women cannot love is shown by the fact that they have no ideal man to correspond with the male idea of the madonna. She does not desire purity or virtue in a man but something else. Man really hates woman, for it is unconscious hatred of his own sensuous nature. His love is his own intense effort to save instead of to nullify her. It is only men who have no great desire for love that wish to see women emancipated. The only real emancipation, however, is for woman to place herself in the right relation to the moral idea, instead of being influenced by tastes, fashions and desire to marry.

Developmental Pathology, by EUGENE S. TALBOT. First edition. Chicago, 1905.

Dr. E. S. Talbot, the well known American authority on all dental matters, has also for many years been an active student of evolution. Although he has not published a systematic treatise, he has done what is perhaps better in bringing together in this volume about thirty of his papers on various topics. Many of them are too technical or too special to be discussed in this *Journal*, and their topics are too diverse to justify us in speaking at length concerning single articles. The data brought together here make this book almost indispensable to any students of evolution.

Étude Psychologique des plus anciens réveils religieux aux États-Unis, par JACQUES KALTENBACH. W. Kündig & Fils. Genève, 1905. pp. 152.

The author is a young Frenchman especially interested in religious psychology, who divided a year between Harvard, Clark, and Chicago Universities in collecting material for this particular study. He does not speak of his special experiences here, but he has studied the literature so effectively and commands it so well that he has told a very interesting story. He first describes the milieu and the antecedents; then the revival from the social point of view from the first period beginning 1735 to the third ending 1734. He then points out the influence of the revival on different ages—childhood, youth and old age. He then, in successive chapters, discusses it from the theological, individual, moral and physical point of view. He believes all religious influences tend somewhat toward revivals and should do so, but that there are special dangers which often neutralize their value. It is dangerous to appeal in a very emotional way to young children. Every revival is dangerous, save so far as it is profound and passionately moral. If it is a mere excitation of sentiments it is liable to do harm. Revivals cannot be immediately connected with any one system of theology, for they are provoked by those that are very different. Societies, like individuals, differ. Some are subject to violent crises and others make progress by insensible degrees. A revival often succeeds when particularly refractory with other church influences. The history of revivals is a great encouragement to Christianity, and in a sense it is a *rapprochement* to the religious state of thought at the time of Jesus. The originality and power of Christianity consists in that it offers us a holy personality to love.

Self-Synthesis, a Means to Perpetual Life, by CORNWELL ROUND. Simpkin, Marshall, Hamilton, Kent & Co., London, 1906. pp. 32.

Self-synthesis is something very unique. We have two instincts—that of self-preservaton or life and a primitive death instinct. These are polar opposites. So man has two nervous systems—the sympathetic involutional and the brain, which is the seat of evolution. There are two minds—the subjective and the objective; three states of consciousness—subjective, individual, and objective. We hover between two worlds—subjective and objective. The relation between these is all-important. Telepathic connections are from above. Women give less suggestion than men, but are more receptive of them. We need much self-protection from adverse suggestions. We should emblazon before us such maxims as, "I feel and look younger day by day." "Health is natural." This, with the avoidance of drugs, deep breathing, plenty of sleep, control of the passions and thoughts, will enable us to be true to ourselves and fulfill the higher possibilities of our nature.

Nerven und Seele, von MAX KASSOWITZ. Allgemeine Biologie, Vierter Band. Moritz Perles, Wien, 1906. pp. 534.

In this fourth volume the author prints some score and a half of lectures on stimulus conductivity and reaction, and in the second part nearly as many more on consciousness. He is a well known physiologist who has attempted to extend his conceptions to the mind. Movements follow not the command of consciousness, but consciousness follows movements. We could never know empty space, homogeneous or resting matter if they existed. Feelings depend entirely upon the sympathetic and vasomotor system. Self-consciousness is an activity of a reflex type, every state of which implies an outer world working upon us. The mode of treatment of the topics in both the neural and the psychic part of this volume is interesting and somewhat original.

Mexican and Central American Antiquities, Calendar Systems and History. Twenty-four papers by EDUARD SELER, E. FORSTEMANN, PAUL SCHELLHAS, CARL SAPPER and E. P. DIESELDORFF. Translated from the German under the supervision of Charles P. Bowditch. Smithsonian Institution, Bureau of American Ethnology, Bulletin 28. Government Printing Office, Washington, 1904. pp. 682.

This is a special study of its subject, illustrated by 134 cuts of various kinds, some of them full-page and a very few of them colored.

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JEALOUSY.

By ARNOLD L. GESELL.

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There is a good, old-fashioned maxim which says, 'Envy no man.' But there are a hundred old sayings which make mock of the maxim. There is a French proverb that 'The envious die but Envy never;' a Latin proverb that 'Envy never has a holiday;' a German proverb that 'No one lives who does not envy;' a Danish proverb, 'If envy were a fever, all the world would be ill!' Another proverb declares, 'There is no man, however high, but is jealous of some one, and there is no man, however low, but has some one jealous of him.' In addition, there are the sour-grape, and the dog-in-the-manger fables, the evil-eye folklore, and a wealth of fairy tales about envious stepmothers and jealous queens. 'Do ye think that the Scripture saith in vain, The spirit that dwelleth in us lusteth to envy?' The subject of our study is something very deep and universal in human nature. Indeed, jealousy is so ancient that it existed before human nature itself, as witness the present day manifestations of this passion in the lower animals.

I. THE PSYCHOLOGY OF ANIMAL JEALOUSY.

When through the stress of competition, animals come to feel antagonism and discomfiture in rivalry situations, they may be credited with jealousy; at least, in accordance with our definition. Omitting all speculation about love, hate, and sex feelings in atoms, crystals or plants, it may be safely said that the three primordial animal instincts about which all others cluster, are the self-preserving, the pairing, and the parental. At the lower end of the animal scale, where Nature is bountiful, where the conditions of life are comparatively simple, these three instincts, so far as they exist, remain comparatively simple, both in their operation and their psychic equivalents. As we approach the vertebrates, however, the creatures and the conditions of their survival become more complex. The fundamental impulses lose their original unblended character, or become associated with complicated activities, developed in response to the more difficult situations. As the biological behavior grows more complex, new psychic states come into existence, and fear, surprise, or anger make their first appearance. Something like a rudimentary jealousy is probably present among the arthropoda. "Even insects," Darwin says, "express anger, terror, jealousy and love by their stridulation." (9, p. 350.) Pocock attributes anger and jealousy to the spider. As many as five male spiders have been observed on one web after the same female, and Dr. Porter reports one case where the male achieved his mate only after vanquishing two rivals in two successive combats. (33, p. 347.) Among the crustacea there seems to be a high degree of emotional life. Pliny credited the pea-crab with jealousy. Lobsters are known to monopolize a special corner of an aquarium and jealously to expel intruders within a certain sphere of influence. Crabs fight vigorously for the same morsel of food, and show a tendency to clutch the morsel and swim off with it, guarding themselves with the free mandible. Hermit crabs often make disturbances in aquaria, because of a sort of restless jealousy which impels a strong individual to leave an apparently good shell and evict perhaps a half dozen of his neighbors from their domiciles, in succession, after as many encounters.

But coming at once to the vertebrates, Romanes is certain that jealousy appears in the very first class of this division. "Jealousy, anger, and play unquestionably occur in fish and batrachia." The stickleback is a good example. He will enter battle against rivals for a female; he will appropriate to himself a portion of the sea and furiously attack any other stickleback who may cross the frontier; changing color from rage, he will seize an enemy by the fins and lash with all his strength when a brood of fry are in danger. (35, p. 245.) The proprietary instinct, as Letourneau has shown, is closely related to jealousy, and becomes well developed among the back-boned animals. The lion assumes a large forest preserve upon which no other may trespass, but on pain of battle; aurochs collectively take similar authority over their feeding area; the pariah dogs of the East have their beats which they strictly police; monkeys and even the birds of the air have their hunting grounds. Besides this ownership in domain, there is the hoarding instinct, found in the rat, squirrel, hamster, and mole, who not only burrow spacious underground dwellings but fill them with a store of winter food. The same is true of birds like the shriker, the woodpecker and the owl, who buries its surplus provisions like the dog. (27, p. 422.) Furthermore, there is a widespread property in dwellings, "The bear has his cavern, the badger, rabbit and mole their subterranean home, the lion his den, almost every species of bird its nest, the beaver its lodge; the dog, fox, coyote, cat. tiger, and leopard all build temporary homes for their young." Among all these animals there is a stern struggle for existence, where appeal must often be made to the ordeal of battle. This property in food, domain, dwellings, and we may add in mates is not inherited, nor does it even come by the mechanical working of a proprietary instinct; it must all be acquired against rivals, and once acquired it must be watchfully guarded. In such a fertile soil of competitive strife, aggresive and defensive, —against fair rivals on the one hand, and intruding contestants on the other,—the feeling of jealousy which is such a useful ally to both offensive and defensive warfare, would strike deep root.

With this general statement before us, we may present in greater detail, cases of jealousy in animals of different species. Delbœuf reports an interesting case from the reptilia, the second lowest vertebrate class.[1] (12.)

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[1] The descriptive accounts of cases of jealousy are printed in smaller type, not because the writer considers them subordinate in importance, but only to differentiate the same from the body of the article, and to economize space. The sex of the cases is indicated throughout by B and G or M and F; the age, by the figures following.

He owned two pet lizards, both males, who lived on the best of terms with one another, and slept side by side, often interlocking. But no sooner was a female introduced than "Pedro conceived a great antipathy for Pierre, which became more evident every day, tormenting him till I was obliged to make a separate cage for him." This is a clear case of sexual rivalry, as is the following of food rivalry. "My lizards did not vary from the general rule. The best worm was always the one that a comrade had. If it was long we might witness such a steeple chase as is seen sometimes in a poultry yard." "Pedro was (also) jealous of my preference and caress. When he was on my sleeve I could keep him for hours motionless by passing my hand lightly along his body; but if I took Pierre or another lizard up, his rage broke out at once and he would jump upon him with his mouth menacingly wide open." This would seem to be a case of rudimentary affection jealousy.

Next in the scale we come to birds, whose jealousy is proverbial. "That they also manifest the kindred passion of emulation," says Romanes, "no one can doubt who has heard them singing against one another." Darwin and Morgan both allude to the fact that song birds, when matched against each other, will sometimes sing to exhaustion and even to death. The rivalry of male birds in courtship is so ferocious that it freqently results fatally to one or more of the contestants. Especially is this true among wild birds, and it has been observed that an ordinary barnyard rooster will tolerate a rival in the yard, whereas a jungle fowl would kill him. But the fierce competition for food so commonly seen in the poultry yard shows that the spirit has not altogether died out of the domestic species. Pigeons kept in a coop will take possession of a certain proportion of perches, nests and space, which they will guard against infringement. Kingbirds and sparrows will also guard domain, and eagles assume feeding grounds. Just as the jealousy of the males flames up in the season of mating, so the pugnacity of the females comes to the surface at the period of brooding. The white shafted fantail "is a lively and amusing little being, singularly bold and confiding in character, betraying little fear of man. These attributes, however, entirely disappear during the breeding season, when the little bird becomes as suspicious and timid as it was formerly bold and confiding. It cannot endure that a human being should even approach its nest." (Wood.) The ordinary barnyard hen in the midst of her brood is a walking incarnation of maternal jealousy. She will even peck at unoffending chicks, which belong to a brood other than her own. But more highly developed, and more difficult to interpret than these jealousies for food, mates and offspring, are those manifestations which proceed apparently from slighted affections. Romanes quotes such a case as occurring in a cockatoo, who showed jealousy "at the sight of his mistress carrying on her wrist, and stroking affectionately, a little green parrot." I shall report only one more such case, which I have directly and in detail, from an accurate observer.

> The bird in question is a small, beautifully colored Cuban parrot, of highly imitative, talkative, and assertive temperament. The affectionate side of her nature is well developed. She was adopted as a pet when six months old, and since then she has had no relations with birds of her own or stranger species, except as she sees them fly past the window, and these she fears. Consequently her affections, so far as she has any, must be expressed with reference to cats, dogs, babies and adults, including especially the members of the household. Those who win her favor are rewarded by a low chattering, or by the gentle caressing of her bill, with which she gently rubs over your eyelashes or lightly explores the chambers of your ears. These are the unmistakable signs of affection. Now the remarkable feature is that the bird is almost whimsical in distributing them; she has her likes and dislikes. With some strangers she takes up readily, with others not at all. For babies she never shows any antipathy; but let her mistress only pet a cat or a dog in her sight, and she is beside herself with rage. Her eyes dart fire, her feathers ruffle, her wings distend, she shoots from her perch and bites the cat or dog with her beak. Cease the provoking petting of her rival and she is appeased.

Now from all the inquiries I could make, there is no other principle of interpretation possible here, but jealousy. Suggestion, imitation, fear, pure pugnacity, or desire for food,—all or any of these are inadequate as an explanation. And what is most conclusive is this fact, namely, that if a *stranger* should pet the rival cat or dog, the jealousy reaction is not as intensely excited as when the favorite mistress of the parrot does the petting. Something like a rudimentary affection jealousy must here be present, and it becomes conceivable when we recall that the parrot by virtue of its gregarious ancestry is an extremely social bird, "and stands at the very top of the whole feathered world for the development of its intelligence."

Proceeding now to the mammals, we are at once confronted with the intense sexual jealousy of the male sperm whales. 'In their battles they often lock their jaws together, and turn on their sides or twist about so that their lower jaws frequently become distorted.' (8, ch. 18.) The next case, cited, is from the ungulates, an unhandsome, but not unconvincing example in the pig. It is taken from Robinson, who has a genius for observing homely barnyard facts. (34, p. 221.)

> "When a pig is alone in a sty he will often take his meals in an indifferent and leisurely manner, and, as often as not, if abundantly supplied with 'wash,' he will leave some of it in the trough until it becomes stale and uneatable. But when several are domiciled together, the beautiful influence of competition, which we so often admire in human affairs, comes to the aid of the farmer. The instant that the pail is emptied into the hog-trough there is an eager rush to the spot, each pig thrusting its fellows aside, and plunging its snout deep into the fluid in order to get as much property as possible into the only strong room he knows of where his goods are in peace."

Even the docile sheep shows the same competitive tendency in feeding. Horses are very susceptible to the influence of pace making, and they, like oxen, are reported to be jealous of favors of food or affection shown to companions. (45, p. 46.) Horses sometimes manifest a tendency to become jealous sentinels, in a manner reminiscent of the ancestral herd life. Thus one old gelding is reported to have made himself vigilant guardian over a young mare, whom he never left while grazing and whom he always protected from other horses. Another horse extended his guardianship over all the young colts in the pasture, and though otherwise of a gentle disposition, jealously repelled all intruding horses by kick or bite.

Some of the most precious examples of animal jealousy are doubtless to be found among monkeys, but the number of trustworthy observations on their emotional life is still sadly deficient.

Lieut. Schipp in his Memoirs (35, p. 493) relates of a Cuban monkey, "He quite understands the meaning of shaking hands. . . . To-day he had been a long time playing with his toys, taking no notice of any one. Suddenly my mother remembered that to-day was my birthday and (for the first time since he came to the house) shook hands with me in congratulation. He immediately became very angry with me, screamed and chattered and threw things at me, being evidently jealous of the attention my mother was paying me."

R. L. Garner, in his book on Apes and Monkeys (p. 163), gives an amusing case of which the following is a condensed account:

Aaron and Elisheba (male and female) are sitting on top of the ship hatch, absorbed in gnawing turkey bones. Along comes a big rival ape. Aaron looks up in surprise, but Elisheba hardly notices him. Big Ape takes seat to right of E. (A. being on the left). When Big Ape is settled, A gets up, walks over, and crowds in between them; then the Ape in turn gets up and deliberately sits on the other side. This performance is repeated six or eight times. A. struck blows at his rival, but in a half jocular, embarrassed manner. He gave no signs of anger, but made no effort to conceal his jealousy."

E. D. Cope, in the American Naturalist (Oct. 1890), relates a case in a chimpanzee, who, when his keeper intentionally neglected him at feeding time to the advantage of a companion monkey, "showed his displeasure by pouting the lips, and finally he would rush from the side of the keeper, and throwing himself on his back would give way to a burst of jealous rage. He kicked his feet, threw straw into the air, and screamed vigorously, the whole proceeding resembling what we sometimes see in a spoiled child. On the offer of renewed attention from the keeper, the chimpanzee was pacified."

It is fortunate that we have with us such convenient objects for study as the cat and the dog, the one representing excellently the gregarious, the other the solitary group of animals. The dog is an especially valuable specimen from our standpoint, since he is so expressive of his emotions, an expressiveness which was developed for its utility in the aboriginal pack life, and has been for centuries conserved by man. The typical dog is by nature such a sympathetic and affectionate animal that he craves attention, and mere indifference or neglect is a punishment; often he loves a caress better than a biscuit. The stimulants to dog jealousy are correspondingly many and various, including not only material benefits, but attentions to all rivals, whether canine, feline, or human. I have noticed signs of jealousy even at the bestowal of mock affection upon an inanimate object like a chair. A fox terrier flew at visitors like a small fury, because they noticed to his neglect, the cunning tricks of a rival. Another male terrier became so dangerously jealous on the arrival of a baby into the household that he had to be put out of the way. Another regarded a pet lizard as a rival and was so keenly jealous that the mere mention of the word "lizard" would rouse the dog to walk excitedly about the room, and bark for as much as ten minutes at a time. Still another dog, who was on friendly terms with a kitten when both were left alone, would interpose if the kitten was petted, would pick her up and deposit her outside the range of the caresser's favor. A contributor to Science (Nov. 25, 1892) reports a case in which the dog did not deal so gently:

> "My brother owned one (dog), a well grown, bright fellow, who was usually upon excellent terms with my kitten, but showed jealousy if the kitten was petted in his presence. On one occasion I held the kitten in my arms and purposely petted and praised it while the dog's eyes kindled ominously at the pretended neglect of himself. Suddenly the kitten jumped from my arms to the floor, and before I could interfere the dog had seized and shaken the life out of it."

Another case, to all appearances as decisive, and the details of which I was able to get directly, soon after its occurrence, may be quoted here for its trustworthiness and instructiveness.

> This time it was a handsome, mahogany-red Irish setter of famous Elcho strain, an affectionate, sensitive dog who lived with his master from early puppyhood. For some ten years, in every weather, it was his habit to accompany his master twice a day, to and from the store, remaining during the business hours near or under the master's desk. But one day a very young kitten strayed into the store. For possibly three weeks it remained hidden under the boxes in the shop, and then, in response to kind treatment and food, cautiously and gradually came out from behind its retreat. During this period of concealment the dog, Rex, who was daily in the shop, must have known the presence of the visitor by the sense of smell, but he evinced no excitement until the kitten had assurance enough to show itself and to play about his master's feet. The kitten was very small, unoffensive, and timid rather than pugnacious, while Rex, it should be mentioned, was a courageous dog who delighted in chasing cats. The master refrained from aggravating any resentment, treated Rex in the usual manner, and did nothing to the kitten beyond feeding it, and occasionally taking it up in his lap. But this was too much for Rex. Day by day he showed clearer signs of his discomfiture, snuffing in a suspicious manner and watching askance for the appearance of the kitten as soon as he entered the store. His accustomed position was

under the desk, near the end of a long, narrow shop; but day by day as the cat emerged from its seclusion, *he* made a gradual retreat. Finally, after about two weeks of this gradual withdrawing, he refused to enter the shop altogether, and remained outside, casting furtive glances through the window. But after a few days he surrendered even this outpost. He would accompany his master every day as usual, but on reaching the nearest corner, fifty yards from the store, he deliberately stopped, eyed his master till inside the door, and then turned home. This was his daily behavior for a long time, until his master gave away the cat, and urged him back into the shop. Thus, almost as gradually as the kitten itself emerged from the background, did the dog retreat from a position he had held for years, finally to surrender it completely to his inglorious rival.

There are many interesting questions arising in connection with manifestations of dog jealousy. What is the earliest age at which a dog shows the first unmistakable signs of jealousy? Six months seems to be a conservative opinion, excepting, of course, the slight amount of jealousy found in the fighting plays. What breed of dogs are most susceptible to jealousy? And why? Is there any difference between mongrels and pure strains in this regard? Dr. Wesley Mills, of McGill University, Montreal, in an interesting letter, from which I take the liberty of quoting, has some observations on these points based on wide experience with cat and dog psychology.

"I think the Irish setter ranks high in affection, but whether the collie be more affectionate than other dogs, he is certainly above all others the dog of his master, and sometimes of one master, that is to say, he never becomes attached to a second master as to the first. Collies are also, perhaps, the most jealous of all dogs. I had in my own kennel some years ago a very remarkable instance of this. A young collie which came into my possession when ten months old, in the course of a year or so developed such jealousy that scarcely a single dog in the large kennel I had dared lift up his head. To such a pass did matters come that, although the dog was a valuable one, both as regards breeding and intelligence, I was obliged to part with him.

"The chief difference between mongrel dogs and pure breed dogs I can best express by saying that a pure bred animal is often a gentleman and a specialist, whilst the mongrel may be compared to a sort of a jack-of-all-trades with considerable intelligence and comparatively little refinement of feelings. The pure bred, I think, feels more than the mongrel, but whether he is more jealous I hesitate to say. The subject is complicated by many considerations involved really in the above general lines of distinction.

"My observations on cats are not so numerous as on dogs; but I have little doubt that they are, to some extent at least, jealous, though certainly not to the same degree as dogs."

To consider a moment this question of cat jealousy. The cat, it must be remembered, is not a social animal like the dog, a difference which is important from our view point, and expresses itself in many ways, *e. g.*, behavior in eating. Quoting from Robinson (34, p. 229).

"The way in which a cat takes its food is a sure sign that, in its natural state, it is not in the habit of associating with greedy companions. When given something to eat, it first carefully smells the morsel, then takes it in a deliberate and gingerly way and sits down to finish it at leisure. There is none of that inclination to snatch hastily at any food held before it, which we observe even in well-trained dogs; nor does a cat seem in any hurry to stow its goods in the one place where thieving rivals cannot interfere with them. Indeed, no greater contrast in natural table manners can be observed anywhere than when we turn from the kennel or pigsty and watch the dainty way in which a cat takes its meals."

This difference in feeding habits is fundamental and very interesting. In the matter of sexual rivalry possibly the cat shows a greater development, while in parental protection there is no great difference. The jealous proprietary and hoarding propensities manifested by nearly every watchdog are, however, largely lacking in the cat. In matters of affection, in spite of its proverbial 'cat love,' the cat's conduct often resembles that of the dog. Jealousy is aroused in the same general manner, but with less frequency, by caressing another cat, dog or human rival. Occasionally cat and dog in the same household are mutually and reciprocally jealous of each other. If Lindsay can be trusted, a mother cat may torment by biting and cuffing, and even kill, one of its own offspring which happens by its cuteness to attract all the attention to itself.

The writer was able, as in the case of the dog, to get trustworthy details in regard to one instructive, if not typical, example of cat jealousy.

This cat was a sturdy, black, double-pawed, male animal, eleven pounds in weight, of affectionate disposition, lively and nervous. Adopted when two months old, a lean, pitiable looking kitten, he grew up under enviable attentions. Says Mrs. X., his mistress, 'I tended him as well as a child, gave him a bath as regularly as I would a child. He took the place of a child to me; but when the baby was born everything was changed for me. I didn't have time for the cat any longer.' It should be mentioned also that the kitten was unsexed when about three or four months old, that he had little society with others of his kind, and that most of his whole existence, emotional and material, centered about the home and about Mr. and Mrs. X., who, as stated, treated him like a child. All this would have an effect on the cat's psychology and help explain his subsequent behavior. When the baby was a day or two old, the cat sniffed up to the cot where it lay wrapped up in a shawl. The cat soon began to growl ominously, to spit, and made signs of cuffing it, but was prevented, and then ran to the door and importuned to get out. After that experience the cat would no longer stay in the house as a regular, peaceable pet, and his behavior was visibly changed. He would scarcely allow Mrs. X. to pet him any more, and would *not* permit her to take him up in her arms. Whenever, for any reason, the baby became the centre of attention, he suddenly went to the door and mewed to be let out. Let the baby cry and he became restless. If the door was not immediately opened, he pawed at it, and raised his voice to a howl, swished his tail, and so persisted that for very nuisance the door was opened. His actions indicated discomfiture rather than rage or grief. Once out, he made for the neighbor's barn, where Mr. X. worked. When Mr. X. returned from his work, the cat followed him home, but never to remain long. With time he grew more and more reluctant either to follow or to stay, and accompanied Mr. X. only half way home. Finally he disappeared altogether. But prior to his final disappearance, there was an event, which increases for us the value of this case. When the baby was four months old, the mother left for a visit and remained away nine weeks (as if on purpose of experiment to put this interesting case to a test!). The cat responded to the test, came back and resumed the even tenor of his previous peaceable ways. But on the return of his baby rival, the place again became intolerable, and he deserted it completely, as related.

Nature and Function of Animal Jealousy. With these examples before us, a fuller discussion of the character of animal jealousy is possible. We defined jealousy as arising from a conflict of interests (rivalry). Now manifestly in animals, the conflicting interests are not reflectively considered or comparatively perceived; in other words we do not have that higher reflective emotion which man can boast, but have instead, something more automatic, something direct and instinctively inherent in the organism; we have, to use Baldwin's terms, organic or instinctive emotion. At bottom, however, all feeling in man as well as in animals is instinctive. Admitting the concept of evolution, we can by no distinction divorce animal from human emotions. We must infer that the feeling life of beasts is, in some dim way at least, like our own. The impenetrability of consciousness forbids our saying or thinking anything very different.

If Nature, by some Utopian adjustment, had allotted but one male to every female, given each pair its measure of land, air and sea, and then put up cordons protecting every family from every family, the rich life of feeling would never have developed. Under such an arrangement, the specific instinctive activities might all exist just as now; the animals would be hungry, would breed and rear their young, but mere Cartesian mechanism would suffice. Jealousy, of all things, surely would not exist, for it is unthinkable without strife. It is precisely because Nature established no such quiescent equilibrium, but thrust her beasts, birds and fish into a rivalry where vigilance was often the price of survival, that jealousy appeared upon this earth, and appeared early.

Says Morgan, "Whatever may be the exact psychological nature of the emotions, it may be regarded as certain that they introduce into the conscious situation elements which contribute not a little to the energy of behavior." Indeed, is not this the very reason for their existence, at least in the animal world? Darwin, once or twice, seems to go even farther in the attribution of utility to the emotions of pugnacity and jealousy, for he speaks of them, not only as being the correlates of male sex, strength, and beauty, but as having a causal effect in producing the secondary (male) sex characters. (8.)

The various emotions, then, are allies, which the agency of natural selection has furnished to stimulate and intensify instinctive behavior. Of two candidates for survival, both equally equipped with the biological modes of action, that one who had the better *psychological* equipment in the way of feelings to energize his action would win the palm of existence. In a state of rivalry between individuals and species the possession of mere ability to eat and to pair could not insure existence. Vigilance, combined with pugnacity, was the prerequisite of survival, and those creatures who had the least amount of vigilant jealousy suffered the 'stern fate of elimination.' Viewed broadly, jealousy seems such a necessary psychological accompaniment to biological behavior, amidst competitive struggle, that one is tempted to consider it genetically among the oldest of the emotions, synonymous almost with the will to live, and to make it scarcely less fundamental than fear or anger. In fact, jealousy readily passes into anger, and is itself a brand of fear.

Relation of Jealousy to Other Instincts. We have spoken of jealousy, both as an instinct and as an instinctive emotion. The latter term may be more satisfactory, because 'instinct' connotes some specific biological activity, whereas jealousy rather suggests a general, plastic store of emotional stimulant. In a few cases, however, the principle of jealousy seems to have crystallized into a definite instinct. The queen hive bee is possessed of an instinct which drives her to destroy the female cells before they are hatched, to forestall, as it were, future rivalry. The common tomcat has been known to strangle the males of his own litter, when they are but a few weeks old, long before they, his potential competitors, are mature. A fledgling cuckoo will eject eggs and nestlings from the home of its foster parents. Again, jealousy is particularized in the sense that the most critical situations bring it out most strongly, and there is always a special increment at the pairing and breeding seasons.

Jealousy, as we have indicated, is closely connected with the *proprietary instinct* in general; it is related not only positively with the desire to appropriate, but negatively with the capacity to feel imminent and actual disappropriation. Letourneau makes both envy and jealousy an exacerbation and outgrowth of the instinct of property.

The relation which jealousy bears to instinctive *sympathy*, would be fruitful to trace out in detail. Sympathy was developed like jealousy as an organic emotion, which furthered the survival especially of gregarious animals. Unlike sympathy, jealousy is anti-social, but not without having a place in the economy of things. "As a complication of sympathy, also considered as instinctive in animals (jealousy)," says Baldwin, "would seem to be a necessary outcome of the law of utility, for the dog whose sympathies for another had no such modifitation would stand by and perish while others lived, whenever the competition for food was sharp. His delight would be to see others eat." (1, p. 235.)

In the course of events it is likely that individuals arose who had a Quixotic fund of jealousy, disproportionate to their interests; but these would perish, and in many species, natural selection, to check such unsymmetrical development produced the instinct of caution and *bashfulness.* Just as jealousy may be considered an antidote to offset the disadvantages of an overdeveloped sympathy, so bashfulness and caution, which are a differentiation of fear, can be regarded as a counterpoise for a too aggressive jealousy. That peculiar form of bashfulness found in the female and called *coyness*, on the other hand, may be regarded as a stimulant to jealousy.

Another influence which has been of great importance in offsetting and reducing the scope and strength of jealousy is the instinct of *mutual aid*, which, as Kropotkin has pointed out, is a factor in evolution on a par with mutual struggle. It is the latter which favors the growth of jealousy, while with the former are associated the amenities of sociability, and peacefulness. To quote a few examples from Kropotkin's work: 'The little Egyptian vultures live in close friendship. They play in bands in the air; they come together to spend the night, and in the morning they all go together to search for their food, and never does the slightest quarrel arise among them.' 'Pelicans fly to their resting place, always the same for each flock, and no one has ever seen them fighting for the possession of either the bay or the resting place.' 'Sparrows announce and share food.' Life in animal societies even develops 'a certain collective sense of justice growing to become a habit.' (29, pp. 22-24, 58-59.) "Separate groups of penguins have separate resting and separate fishing abodes, and do not fight for them." Bank swallows are very congenial, even when there are more birds than resting sites. The migration of birds also furnishes fine examples of co-operation and sympathy.

Adolph F. Meyer, a sympathetic but careful student of bird life, writes of the white throated sparrows (during migration) as follows: "They are about the most loving and peaceful birds I know,—like the swallows but with more 'soul life.' I can imagine no more impressive scene of loving tenderness than a flock of white throats cuddling together in a brushheap at eventide and with soft brooding notes of cozy companionship going to rest for the night. This scene, and their rather soft tremulous song, have so endeared them to me that I can hardly imagine white throats ever to be jealous."

Here, then, in sociability and mutual aid we see the other side of the shield; but jealousy, however anti-social it may be, retains a function in zoölogical economy; namely, to conserve the individual as against the group. It is Nature's great corrective for the purely social emotions.

In this connection arises the interesting question, whether solitary or social animals have the greater development of jealousy. The question is complicated by the fact, as Kropotkin has pointed out, that "it appears probable that apart from a few exceptions, those birds and mammals which are not gregarious now, were living in societies before man multiplied on the earth and waged a permanent war against them or destroyed the sources from which they formerly derived food." (29, p. 52.) It seems, also, that solitary animals will combine on occasion, as lions in hunting. Gregariousness increases the general amount and expressiveness of emotion, and develops sympathy and sensitiveness; this, combined with the fact that social animals are more frequently confronted by rivalry situations, would tend to the conclusion that they are more jealous. They surely have more capacity for affection jealousy, as witness the parrot, crane, and dog, and we have already referred to the jealousy of canine 'table manners.' In regard to sexual rivalry there appears to be no such difference. The number of cats observable exceeds the number of dogs, and the fact that they are much less frequently credited with jealousy is significant.

Mutual aid within a gregarious group by no means excludes the operation of mutual struggle with its attendant jealousy, and Kropotkin himself cites the quarrelsomeness of the social rats in our cellars, and of the social morses, which are prone to fight for the possession of a sunny place on the shore. Moreover, it should be noted that the operation of the mutual aid law is confined within a group, or a species, and does not prevent but often intensifies the struggle between groups and species. Thus sparrows may share food, but they will fight with fierce jealousy to keep a domain free from strangers.

The Factor of Sex, like that of sociality, is difficult to evaluate. In the matter of food rivalry its influence is slight or nothing. In sexual rivalry, jealousy is largely confined to the male, except in such few cases like the cassowary, mentioned by Darwin, 'where the female takes the lead in courtship, and plays the part of the male throughout, exhibiting all the worst of his passsions, such as rivalry, jealousy and ferocity, the rivalry and jealousy leading to frequent battles for the possession of the male.' But whatever effect the mating season may have in increasing the jealousy of the males, this is partially compensated for by the demands which the breeding season usually makes on the female in watching over and protecting her young. Finally, it is well to note that jealousy is exhibited in unsexed animals,—emasculated cats, spayed bitches and geldings.

Expression of Animal Jealousy. There are two characteristic modes in which animal jealousy expresses itself,—excitation of the creature, or depression. Just as in the social human there is what has been called 'a sense of other persons,' so in the higher animals there is 'a consciousness of kind,' and an instinctive sense of a rival's presence. The experience may be momentary or prolonged; if it is colored with the proper stenic or asthenic reactions we have jealousy. A worm is thrown to a couple of Lloyd Morgan's chicks. "Instead of quietly and leisurely dealing with the worm in accordance with its special meaning as it (a chick) does when there is no rival in the field, the chick darts at it and bolts with it in accordance with the special meaning which its neighbor's presence under such circumstances, has acquired. And this difference is in the conscious situation,—the interest of which is centered in the companion."

In the lower and more usual forms, the expression of jealousy is of this positive, aggressive nature. First comes the perception of the situation of rivalry, and then with instinctive promptness, for the reaction is organically ingrained, the bucks lock horns, the chicken bolts, the fish makes a lunge, etc. There is an instantaneous toning up of the muscular system; the heart beats faster, blood sets to the head, breathing accelerates, the eyes kindle and the whole organism tingles ready for onslaught. If the onslaught immediately follows, the jealousy passes into rage and we have the jealous rage or raging jealousy; if not, we have what appears to be a painful state of 'obstructed conation,' of 'thwarted impulses,' of nascent fear and anger, or of vigilant preparedness.

As the situations become more complex, and as the animal's emotional nature becomes more highly developed, the jealousy reaction is not always simple. Take dogs, *e. g.* The manager of a Pennsylvania kennel writes me as follows: "The different modes of expressing jealousy by dogs and other animals, we consider depends on the disposition of said animals entirely, as you will find almost as many different dispositions in animals as you will in persons. Through jealousy, a dog, for instance, if he be of an aggressive nature, may become very much so toward the object of his discomfiture, but if he is of a tender nature, and much devoted to his master, he may through jealousy, just sit and cry or go away and mope by himself over his grief and in some cases refuse to eat." Almost any one can substantially verify the statements of this observer. We should insult the psychology of the dog, if we expected the same stereotyped jealousy reaction in the ponderous mastiff, the leisurely spaniel, the dainty lapdog, the nervous terrier, and sensitive collie. Moreover the individual differences in specimens within the same variety, yes, the same litter, are marked. Anger symptoms are, naturally, the most common and need not be reviewed, but in addition to angry biting, etc., we have whining, howling, whimpering, moaning, subdued barking, pawing, rubbing, retreating, nervousness, nosing, squirming, restless pacing, frisking, performing of tricks as though to attract attention, interposing with paws, slinking, hiding, crowding, licking, swallowing (apparent lump in throat), dropping of head, tail and body, rolling of eyes, loss of appetite, moist eyes, tears,—and combinations of expressive movements difficult to describe, but which are popularly made to indicate moping, despair, dejection, sorrow, sulking, etc.

The group of depressive symptoms taxes our power of explanation more than the excited. To say that the former show wounded feeling, or slighted affection, perhaps connotes too much. Yet they plainly bespeak a painful emotional state. Skye terriers (one of the most jealous breeds) have been known to shed tears freely, and instances of dogs pining away from grief, in such cases are reported. (14, p. 181.) That they refuse food is beyond doubt, and I have known of at least one dog with ample opportunity to eat, who grew fatter as soon as his rival left the premises.

An interpretation of animal jealousy will be attempted in a future section, where the genetic development of this feeling is traced. Tentatively, it may be said that if animal jealousy cannot be reduced to anger and fear, it at least contains these as prominent elements. In the most rudimentary form jealousy is almost indistinguishable from pugnacious anger. In the more developed type represented by the dog who cries, whose whole demeanor is one of depression, we are at the other pole,—anger has given way to fear, which is one of the most depressive of feelings. If fear is made to include not only terror and timidity, but also the feeling of helplessness, and the sense of loss or defeat, it proves a still more useful principle of explanation; and the expressive movements manifested in depressive jealousy become thereby more comprehensible. They may then be interpreted as remnants of what were once utility reactions. Slinking, hiding, retreating and sulking represent the fleeing to safety from a feared or victorious rival; crouching is a sign of submission; the drawing in of ears and tail was once a highly serviceable attitude when in danger of tooth and nail; whining and howling are the language cry of distress; while the restless and askant eyes, so characteristic of jealousy even in man, are reminiscent of the furtive, hurried glances at a pursuing or prowling foe. There are border line cases where the askant eyes bespeak anger rather than fear, where, by his nervous, embarrassed behavior, the dog seems hardly to know whether to fly at or from his rival. Which again goes to show that we are dealing with a plastic anger-fear psychosis.

However complex, in its higher states, human jealousy may be, it will always be found to bear 'the stamp of its lowly origin;' and will become more comprehensible in the light of its pedigree. It is significant that in the prehuman jealousy just surveyed, there are revealed the same two large types which the emotion assumes in man: the excited and the depressive,—the jealousy which is angry and the jealousy which suffers.

II. THE PSYCHOLOGY OF HUMAN JEALOUSY.

There seems to be something uninviting or sinister about the theme of jealousy. Stanley in his comprehensive 400 pages on the Evolutionary Psychology of Feeling does not so much as mention the word jealousy. Ribot deplores the scanty literature on the subject, but himself devotes only a few sentences to it in his book on the emotions. James dismisses it with the one line namely, "Jealousy is unquestionably an instinct." Even Darwin has little to say on the matter. While anger and fear have received considerable treatment jealousy does not even have a monograph. Yet this feeling is both biologically and psychologically interesting, and is laden with practical importance for criminology, sociology, and even pedagogy.

A questionnaire was issued last year by the writer for securing certain data upon which to build some genetic and psychological interpretation of the subject. The syllabus follows:

A. *The rôle which envy and jealousy have played in your life.* Give a running account based on retrospection and the testimony of your parents, describing the manifestations of envy and jealousy in (1) Infancy, (2) Childhood, (3) Adolesence. Make a full list of the things which have prompted your envy or jealousy (not omitting the most trivial) and then select a few specimen cases for frank exposition. Tell at what age outbreaks first appeared, when most frequent and intense, how long they lasted, how they felt, how they were appeased, what actions they led to, etc. Have you ever felt envious or jealous of the opposite sex, or of persons in literature or history? How do you feel toward a person whom you know to be jealous of you?

B. *Envy and jealousy as observed in others.* Describe mean, malicious things done by infants, children or youth from jealousy (such as destruction of toys, snatching sweetmeats, slapping, spitting, injuring clothes, disfigurements, lying accusations, etc.). A detailed description of some striking incident will be especially valuable. Describe also, if noticed, less aggressive manifestations like sulking, crying, hiding, etc. Mention the occasion for the jealousy, the approximate age, and the sex in each case. In the absence of overt acts are you able to detect envy or jealousy? How?

Give cases of 'class jealousy' which you have seen between groups of children (wards, neighborhoods, streets, clubs, etc.), between churches, societies, business or other corporations, cities, families, etc.

Add, if possible, observations of 'jealousy' in lower animals, stating sex, circumstances, outward expression, actions prompted, etc.

C. *An introspective description of the state of jealousy as you now know it.* Tell exactly how you feel (which, if any, of the following feelings are present: anger, anxiety, grief, rancor, self pity, fear, mortification, desire to injure). Tell how the attack comes on (slowly or suddenly); when most likely to occur; how long it lasts; whether the same attack recurs often; what impulses, what thoughts inspired. Describe all outward or inward expression, *if any* (flushing, paling, chill, sweating, changes in muscle tension, scowling, grinding of teeth, movements of eyes and tongue, clenching of fists, attitudes of body, stamping, etc.). How do you feel after an attack? Do you exercise any means of control, and with what success?

In what respects was the state of jealousy different when you were a child?

If you make a distinction and find noteworthy differences between the state of jealousy and that of envy please state same.

D. *A description of the 'jealous temperament.'* Delineate an individual who is distinctly of a jealous disposition. Tell the nature of the jealousy, and note whether the person is otherwise selfish, genial, generous, irritable, sluggish, melancholic, energetic. Specify age, sex, nationality, and any pertinent facts of heredity.

The syllabus was by no means a popular one, but 345 persons kindly responded, most of them with satisfactory frankness. The bulk of these replies came from young women students of the New Paltz, N. Y., and Trenton, N. J., normal schools, and from advanced students of both sexes in Northwestern University, Evanston, Ill. Madame Anna Grudzinska, of Kieb, Russia, sent a valuable communication accompanied by excellent replies from ten Polish men and women (average age over 30, and mostly married). Post graduate students from Clark and Columbia Universities also responded. In addition to the reminiscent and introspective accounts, 450 observations on children, youth and adults were collected, so that in all over 1,000 cases were available for our study.

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Infant Jealousy (from birth to six years of age). Having considered the animal aspects of the subject in a previous section, and reserving the treatment of jealousy in primitive races for a later section, the topic next in natural order is lent outbreaks of jealousy. The manifestations are very pronounced and they seem to be especially instinctive in origin. They constitute, sometimes, the most precocious of juvenile murders. And, to look at things from the child's standpoint, the provocation is great. After an uninterrupted and absolute reign, to take a little monarch by the hand, and with mysterious mien to usher him into the birth chamber, and ask him to felicitate on the arrival of that ugly looking little pink mass, there on the bed, robbing him of his accustomed caresses,—that is asking too much. And the scenes which are enacted on these occasions are not without their pathos and tragedy.

B 2½ showed a contorted mask of pain, spiritual discomfort, and almost despair on the birth of his brother. G 5 cried, 'Take him away. She is not his mama, but all mine.' B 4 showed jealousy for a whole month after the birth of his baby sister; refused to leave his mother and had to be forced out of the room; would not play; would not allow any one to touch him in these spells, but cried continuously, "I want mama, I want mama!"

Helen Keller in her autobiography makes the following confession: "For a long time I regarded my little sister as an intruder. I knew that I had ceased to be my mother's only darling, and the thought filled me with jealousy. She sat in my mother's lap constantly, where I used to sit, and seemed to take up all her time and care. . . . At that time I had a much-petted, much-abused doll. . . . I had dolls which talked and cried and opened and shut their eyes, yet I never loved one of them as I loved poor Nancy. She had a cradle and I often spent an hour or more rocking her. I guarded each doll and cradle with the most jealous care; but once I discovered my little sister sleeping peacefully in the cradle. At this presumption on the part of one to whom as yet no tie of love bound me, I grew angry. I rushed upon the cradle and overturned it, and the baby might have been killed had not my mother caught her as she fell." (26, p. 15.)

Dolls are in the same category as persons at this and a later period. One, G 2, exhibited jealousy when her doll was petted by her mother, and threw the doll on the floor in a fit of anger. Moreover, as Ellis and Hall found, in 77 of 579 cases, jealousy was one of the psychic qualities which children attribute to their dolls.

Though even infants may cry in secret from jealousy, it is evident from the examples above that their feelings are usually promptly and plainly expressed. Those cases in which the jealousy is formulated in words are especially interesting and convincing.

G 3 on seeing her mother pet another child exclaimed, 'Bad boy mama. My mama.' G 1½ when mother took up her baby brother said, 'Let him cry. Take me.' B 2½ having smashed a rival's toy, said, 'Now I guess you won't play with it.' B 5 joyfully greeted the announcement of his little brother's illness with, 'Now I can have both chairs!' G 4 after her father had carried her brother "piggy back" said, 'I wish I could poison Thomas, I hate him so.' Another little girl, when her mother petted some one else, sobbed, 'All my mama, all my mama,' and would be comforted only when assured that mother was all hers. B 2 clung to his mother on the coming of a baby visitor and could not be coaxed to play. Very seriously he asked, 'What baby is that? Is it coming again?' B 3 when invited to his playmate's party was woefully surprised to see so many present, and staunchly said, 'I don't want other boys around when I go to see Janet.' No mention of ice cream or games could induce him to join the party. G 3 on seeing her lady friend, Miss M., walking with another little girl, said, 'I hate Miss M. I'll kill that little girl that is with her.'

Childhood Jealousy. (*Age, six to twelve.*) This is the elementary school age. The social interests widen, and the sense of self develops correspondingly. Competition, leadership and organization become important features in the plays and games; and the child's whole environment grows complex. The jealousies though still childish are less so than in the previous period; the self takes on, to a higher degree, the character of a socius and becomes more sensitive to fine distinctions. The situations which excite to jealousy are less egregious and often involve only the slightest partialities, or even imaginary differences in favors, gifts, clothes, etc. Let any one attempt to distribute Christmas dolls in the children's ward of a hospital, without taking the precaution to dress all the doll's alike, and he will see what is meant.

As the social sense grows keener, the tendency of rivalry increases. This is the period of boasting and of showing off. The writer examined 105 questionnaire returns on the subject of bragging and taking a dare, and found that these egotistic tendencies in children are by no means purely egocentric, but are colored by a strong social appreciation, and a spirit of competition. The common expression of these boasts is, 'Mine is nicer than yours.' 'Mine is bigger than yours.' 'My pa can lick your pa.' 'My gang can put it over your gang.' A gentleman tells G 6 what a big dog he has at home, and she, not to be outdone, says, 'O, that 's nothing, I have a dog at home as big as this room, and with a head as large as the piano.' B 8 heard his teacher say that a certain girl could read better than he. After school he came up to her and said, 'If you can read better, I can fight better,' slapped her and ran away. Many more instances might be cited to show that it is a social setting which brings out the dare and the boast. The comparative instead of the merely individualistic manner of statement, shows that a spirit of rivalry is behind them, a desire not to be outdone, something akin to jealous self-regard. Hall and Smith in their study on Showing Off say that, "In bragging lies, in regard to possession, envy and imitation are frequent motives." (20, p. 16.) The delight in exciting envy, also, is one of the motives of children's teasing, while precociously coy little maidens like to excite jealousy in their lovers by being nicer to other boys.

Indeed, this is the age when most of the cases of pre-adolescent affection appear, bringing forth a type of jealousy much more highly developed than anything in the earlier period. "Jealousy is present from the first," says Bell. "It is more pronounced in the cases of love between children and adults of the opposite sex, on account of the child's being less able to monopolize the attention of the adult and on account of the precocity of the child concerned in such cases." (5, p. 335.) These youngster lovers are watchful, exacting, hostile toward rivals, heartbroken at slights, and even romantic, as is abundantly proved by the following extract from a letter by a boy of twelve to his sweetheart of eleven:

> G. and N. B. liked to scare me to death and I started to cry. I thought for a while you and Floyd had up a case. But he can't cut me out, can he, Hildegarde, dear? He is jealous, but he can't have you, can he? Floyd makes me tired with his songs and sayings. But do not get mad at me, for I am not mad at you. You and Floyd are not going to get up a case, are you, dear? For I love you, and I think you love me, don't you dear? It makes me mad to see you laughing and smiling at each other. So don't get mad at me, and just tell Floyd you are not going to have anything to do with him.

This is, also, the period of heightened clothes consciousness, and sensitiveness to inequality in dress, which causes many malicious acts traceable to nothing but envy and jealousy. Both boys and girls, as our returns show, attack each others' clothes by cutting holes in them, throwing mud, or spilling ink on them.

> G 6 hid when her sister had a new dress. Another girl led her playmate, who had on patent leather shoes, into a mortar bed. B 11 cut a belt from another boy's coat because he had no belt on his. B 9 cut a slit in his brother's nicer coat. G 11 will stay indoors in the nicest weather, if a visiting playmate is better dressed, and makes bitter remarks from jealousy. G 10 soiled the apron of another girl, and when asked why she did so, replied, 'Because I never had a nice apron.' The spirit of boast and display in boys with new caps and girls with new sashes helps to excite this childhood jealousy to its malicious deeds of mutilations and soiling.

The school life with its examination marks and craving for the teacher's favor gives much opportunity for the exhibition of jealousy. This is the age when tender-hearted girls, especially, become very sensitive to criticism and will weep bitterly if in their fancy another has displaced them or outranks them in the teacher's favor. It is for many one of the keener sorrows of childhood, this inability to secure the predominant if not exclusive love of the teacher. The recognized necessity of observing very strict impartiality in school government indicates that the latent spirit of jealousy is strong.

G 10 marked up her rival's note book on account of the praise which the latter received from her teacher. B 6 slapped a schoolmate 'Cause he always knows his spelling and gets a hundred every day.' G 6 if she is excelled in writing is enraged by her jealousy; will try to sponge out the work of others, to scratch them; and will lie down in the grass and kick and cry because she cannot jump as high as her mates. G 8 will sulk, act stubbornly, will not listen to the story lesson, if she cannot sit next to her teacher in Sunday school.

The type of jealousy experienced by different children, varies not only with their age but with their temperament. Some manifestations show a refinement of feeling which is almost precocious, others are infantile in their crudeness. The few following examples will speak for themselves:

G 8 when washing dishes washed her own cup before her sister's; she wished to stand first; it was an action merely symbolic of her thoughts.

G 10 (reminiscence of college woman). My first experience of jealousy, which made a vivid impression, so that the memory is perfectly distinct after many years, came in connection with an illness of my mother who was my childhood idol. . . . My jealousy was because these kind neighbors insisted on doing things for mother (I wanted to be the sole nurse), and I shall never forget my intense anger at them, nor my fits of passionate crying whenever one of the neighbors replaced me in the sick room. This occurred whenever mother was sick for several years; and I was a girl of 15 or 16 before I succeeded in mastering these jealous feelings.

Dr. Caroline A. Osborne carefully observed for a long period two interesting cases of jealousy in the children's ward of the Worcester Memorial Hospital, and kindly furnished the facts set forth below.

Alice, a beautiful little French girl, having drunk caustic lye, was brought to the hospital at about the age of five, and remained for several years. Both from a medical and a psychological standpoint she was an interesting patient. She had to be fed by a stomach tube, and was much below normal size on account of limited nutrition. Because of her beauty, her diminutive size, and her winning, flirting ways, she was the favorite for the attention of visitors. She had a veritable greed for such attention, and posted herself where she would be most likely to see the largest number of people. A typical child in most other respects, she was precocious in the over-development of a calculating acquisitiveness, which expressed itself particularly in the direction of food. This insatiable hunger, especially for candy, may have been the physiological outcry of a starved system, but it was also a psychological fact. Exceedingly observant and retentive of the minutest details bearing on the great question of food supply, and without a moral sense to abash her, she regularly diverted so far as lay in her power, the generosity of nurses and visitors from others to herself. Her jealousy was surely describable by Lombroso's terms, as "a tyrannical desire to monopolize." It was not a jealousy which expressed itself either in grief or anger, but rather in a cold, vigilant Machiavellianism, which seemed uncanny in so small a child.

Mary Jane, an Irish girl of ten, on crutches from hip disease, was another patient in the same ward. She was of a different stamp from

Alice; overgrown, rather than undersized, appealing to one's pity rather than admiration. Her jealousy also was of a different stamp, and showed itself in outward signs, while Alice's was concealed. It was a common sight to see her standing off to one side, watching with wistful, askant eyes, the attention which the other patients, particularly Alice, so disproportionately enjoyed. This watchfulness was written in the expression of the eyes, her scorn in a drawn mouth with corners turned down. Resentment and a questioning inability to comprehend the situation were also present in this frequent 'jealous look,' as the nurse called it. That it was a real case of jealousy was pretty clearly proved one day, when the nurses, suddenly aroused by a noise, rushed to a scene of commotion in the bathroom. What should it be, but Mary Jane braced on her crutches, vigorously trouncing the diminutive Alice, —and why? Mary Jane explained in a tearful voice, "Because she is so pretty and they give her things all the time!"

Not counting reminiscences, our returns report 151 cases of childhood jealousy, stating age, sex, and cause. The facts which come out most strikingly in these cases (64 boys, 87 girls), when compared with those for the infancy period, are the greater diversity and refinement of the exciting causes, and of the manner of expression. Fully 80% of the infant cases were expressed in fundamental, and what might be called racial ways, — by stamping, screaming, biting, striking, etc. Such manifestations, although they are by no means absent in children from six to twelve, are often displaced by expressions of a psychologically higher order, like sulking, lying, mocking, slandering. Threats of murder and drastically disastrous wishes against rivals may be openly or secretly held, but instead of overt aggression, we frequently find children sulking, brooding, withdrawing into themselves, and hiding sometimes for hours. There are real cases of wounded affection and of bitter, non-petulant weeping for one thing. Tattling also becomes a prominent manifestation. This tattling is a species of contempt, a kind of disparagement, comparable to the more scientific gossip of adults, and children take a corresponding delight in it; because they feel that to lower the reputation of a schoolmate is to increase their own. Again, jealousy comes to be a prolific source of children's lies; tattling itself is often sheer lying accusation. 'Ferriani personally studied 500 condemned juveniles with reference to their lying habits, and found that 195 lied from jealousy, envy, and revenge.' (19, Vol. 1, p. 352.)

Adolescent Jealousy. Eighty-two cases of a reminiscent character, stating age and circumstances, were given. Of these, 50 speak of talents, class-marks, beauty, popularity, etc.; 32 of attentions to friends, lovers, etc., as being the cause for jealousy. The returns show a marked change over the childhood period. And this we should expect, knowing that psychic adolescence is heralded by 'all-sided mobilization,' that 'the consciousness of childhood is now molted,' that a special consciousness of sex now dawns, that hero worship and a longing to excel arise. "Self-feeling is increased," writes Dr. Hall, "and we have all forms of self-affirmation. The new sense of self may be so exquisitely delicate that a hundred things in the environment that would never rankle before, now sting and irritate." (19, Vol. 2, p. 79.) Mere possessions having only a personal value, are no longer causes of jealousy, and if clothes are mentioned it is rather because of their importance in the eyes of others, for the ego now is much more of a socius and keeps at least one eye on the *alter* of society or the opposite sex. Youth desires fine clothes, talents, advantages, for their impressiveness upon an onlooking world. Thus at the adolescent stage the returns mention as excitants to envy or jealousy such things as talent in music, popularity in athletics, brown eyes, girls with long, thick hair, elocutionary ability, a pretty face, self-confidence and powers in conversation, capacity to think quickly, good vocabulary, pleasing, graceful manner, cleverness, skill in making witty remarks. Is it not significant that the more purely personal qualities such as modesty, cleanliness, sincerity, are never mentioned as causes of envy or jealousy? This is the romantic period, when girls especially magnify their own deficiencies and their own possibilities and revel in the mere delight of being a French countess, a colonial dame, a Southern belle, George Eliot, Evangeline, or Joan of Arc. The intense longing to be a great actress, to be a world favorite musician or artist often verges on the morbid, and is then more akin to envy, than to simple, normal desire. An almost constant craving for sympathy and recognition, a new sense of self-importance, and by Hall's law of emotional antithesis, a tendency to pass to the other extreme of self distrust, make adolescence a productive season for rank growths of envy and jealousy.

Youth sometimes appears to be as tyrannical in regard to attentions as infancy, but slights wound in a deeper and peculiar way.

G 17 cried bitterly because her sister entertained a gentleman friend to her seeming neglect. G 13 was hardly civil when her father showed hospitality to a girl visitor of about her age. When the visitor had gone she made her father tell her over and over that he loved her best. G 20 cried a half hour when her sister received a new hat from her mother, thinking it a sign of preference on the mother's part. G 12 threatened to kill herself because of the greater attention she considered was shown to her brother.

If anything, there is more exaction in regard to the attentions of friends outside the home. This comes to light in the crushes and mashes which are now so common. Girls 'chum it' with much intensity, and often live in constant trepidation lest their friend show more preference for another, and ask time and time again to be assured that they are the first and most dearly beloved. This comes out prominently in the returns. Girls are 'terribly' slighted if not told a secret, or a piece of news first, or are not invited first, etc.

G 19 says, "I want my friend all to myself." G 20: "I want my friends near and want to monopolize them." G 16: "I saw a girl with her arm around my chum, and I was so jealous I went up stairs to have a cry." G 18: "I feared lest my friend should love another more, though she constantly tells me she loves me better than any other person." Many of the quarrels and fallings out in these crushes have jealousy at the bottom.

Most intense, however, is the jealousy which accompanies adolescent love-making. Girls like to provoke it in their beaus and play with it, though they are by no means free from it themselves and are fearful when a supposed rival appears with a new trinket, ornament, or a new dress calculated to attract more attention. Two cases are reported below; further examples may be found in Dr. T. L. Smith's article on Types of Adolescent Affection.

M 28. "I had a desperate siege of love jealousy in my high school years. I aimed to look unconcerned before my rival, but tried to get my girl's sympathy by catching her eye, looking grieved or hurt, and simulating a nervous tremble while I wrote. No matter where they sat I kept my eye on them closely; would grind my teeth and utter threats under my breath. The jealousy was a kind of chronic obsession; but was intensified by a concrete situation which I could take in directly with my eyes, when I felt enraged enough to chop off my rival's head. I never planned to poison him, but wished him all manner of evil, and if the circumstances had been right, I might have done something serious. I brooded and would lie awake nights to roll my pain like a morsel. I planned to ruin my rival in the girl's eyes, and contrived to get a blank marriage certificate which I filled out with his name and hers, counterfeiting my rival's handwriting; but was restrained from putting it on her desk by a sense that it was a contemptible trick. One time I caught him when he put his arm around her — I could have killed him on the spot."

The following are extracts from the Journal of Marie Bashkirsteff. (3.) As early as page 2, when she was 12 years old, she says:

I love the Duke of H. Mar. 14. I should be so happy if the Duke would only take notice of me, and I would bless God. Oct. 13. I was looking up my lesson to-day when little Herder, my English governess, said to me, 'Do you know that the Duke is going to marry the Duchess M—?' I put the book closer to my face, for I was as red as fire. I felt as if a sharp knife had pierced my heart. I began to tremble so violently that I could scarcely hold the volume. I was afraid I was going to faint, but the book saved me. I said my lesson in a voice that trembled with emotion . . . . but what passed within me, in the depths of my soul, no one shall ever know! Oct. 17. I read the announcement mention in the newspaper. . . . I could not write in the evening. I threw myself on my knees and wept. (Inserted note: Reading this seven years after produces no effect on me whatever). Oh, I detest him! I want to see them together. . . . How I am changed since the 13th of October, that fatal day! Suffering is depicted on my countenance. His name is no longer the source of beneficent warmth. It is fire; it is a reproach to me; it wakens jealousy and grief within me. Nov. 29. I am tortured by jealousy, love, envy, deceit, wounded vanity, by every hideous feeling in the world.

There can be no doubt that at adolescence the feeling of jealousy rises to a higher plane. Ninety persons definitely answered the question, In what respects was the state of jealousy different when you were a child? With few exceptions, the judgment is that childhood jealousy is more explosive, violent, shorter, more sudden, objective, frank, and is prompted by pettier and more material causes. Following are some of the differences noted in the returns. In childhood it was 'unreasonable,' 'more like a summer shower; now it is a winter's gloomy storm.' 'The attacks were brief, soon forgotten, and did not fatigue so much.' 'In childhood bitterness was felt toward the person, now it is toward the self.' 'As a child there was only egoism in the feeling, now there is none.' 'The signs were more outward, now they are more inward.'

The latter distinction is valid for a good many cases. The typical expression for childish jealousy is violent, overt, anger or aggression; but in adolescence, under the whip of jealousy, the feelings may become painfully subjective; the soul turns in upon itself, broods, grieves. Lancaster found that the curve of despondency beginning at eleven rises steadily and rapidly till 15, and culminates at 17. (19, p. 77.) Only animals high in the scale can experience the depressive type of jealousy, which even in the human first comes to its fuller development as late as adolescence.

Adult Jealousy. Proceeding now to adult jealousy, we are at once confronted with the immensity of the subject and the impossibility of surveying it. The writer, at least, believes that jealousy is an almost universal instinct determining action in a multitude of ways, if not always as a conscious motive, nevertheless pervading the ins and outs of little things and great, from the peanut stand to a continental railway system, from a sewing circle to the Congress of Vienna. The causes and circumstances are too numerous and diverse to invite classification, but it is certain that the instinct of jealousy, however lowly its origin, is not a transitory affair peculiar to infants, children and youth, and that it persists into old age, even after the battle of life has been lost and won. With comparatively few exceptions, in neutral or rare individuals, it is present or latent in all human beings,[1] and colors the struggle for existence in the smallest as well as the largest of its aspects.

Examples of adult jealousy are known to every observer of human nature, yet the citation of a few selected (non-criminal) cases may serve a purpose.

F — idolizes her son, and is extremely jealous of all his friends. Reads all his letters that she can get hold of. When he became engaged to be married, she became ill over it. Though the girl was an estimable one in every way, she persisted in saying contemptible things about her future daughter-in-law.

F 31. I detest now when somebody kisses or fondles my children, and when they cling to friends or relatives; even their devoted love to their father at times grieves my heart.

F 35. I was exceedingly jealous when in love; and tortured the man so that he was quite unhappy. Once married I did not feel jealousy any more. I liked to see him young and charming, and hear how ladies favored him. I often heard the nonsenical question, 'You are not jealous? And you have such a beautiful husband!' I think people and the world often inflame this bad feeling. In later years I often felt envious of wealth, the world's goods and fortune, and wished the worst things to happen to our happier neighbors. The feeling passed with the first stroke of good fortune.

F 23 is a tyrant to her husband, who does not dare to go out without her, to look up or say a word about a woman. She dismissed all the women servants and causes a row about every day. Her jealousy is a veritable madness; her body becomes rigid and she is seized with cramps. Possibly it will pass when she becomes a mother.

F 27. When I was grown up I loved to tease the men who courted for my hand; it was a very agreeable thing to do, and I felt never a moment's remorse. Maybe, it was due to the influence of my old nurse, who always recommended me to torment gentlemen, 'for they are like dogs, — nothing can harm them.' She sang a couplet to this effect, and I thoroughly believed her. I am a jealous wife, I know. I never allow pretty girls to come near, have old servants, and a superannuated nurse for my boy, and she sees to it that I have no cause for suspicion. Now since I have the baby, I am less jealous of my husband, yet I cannot say I am altogether free from this feeling.

M 30. I feel envious in my career now and cannot bear that one speaks about other physicians as able and renowned; it makes me sick at heart.

F 35 is a beautiful and much admired woman, but so jealous that her husband doesn't even dare to go anywhere accompanied by her, because she is sure to make a scene. She hates all women, especially girls. She was also a very jealous child, as her mother says, and had convulsions which occurred every time she had a fit of jealousy. She is consumptive now and we have all observed that since this illness, she is less jealous and even likes it when her husband goes to see her friends; and now, for the first time she has taken a *young* governess into the house. The others had always been 'monsters,' or gentlemen.

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[1] It is, of course, impossible to get any satisfactory data as to the universality of jealousy. Only 26 out of the 346 answering our questionnaire disclaimed having jealousy in their composition. Two hundred and thirty persons replied to the question, How do you feel toward a person whom you know to be jealous of you? Again only 26 replied that they had never known any one to be jealous of them, which is another interesting sidelight, showing the prevalence of jealousy.

F 40 is so jealous that she never allows her children even to embrace her grandparents. One time when her little boy of six years let it out that he had been to his grandmother's and gotten some candy from her, she gave him such a slap in the face that the child actually lost consciousness.

M 28. When in college I became passionately attached to my roommate, in fact I loved him. I did not want him to marry, and I planned a home in which he was to live as one of the family, my wife loving him almost as much as me. When, one day, a letter came announcing his marriage I felt terribly hurt—hurt is the word; it was hardly mortification, still less rage or resentment; it was keen grief, falling sensation in the stomach, pressure pain on the chest, more especially about the heart; it was a dire sense of having lost something, of being poorer. One of the hardest things I ever did in my life was to write the letter of congratulation.

M 70 (condensed delineation of a jealous Scotchman); not selfish; generous to a fault; keenly sympathetic; kind hearted, genial when things go well; quick tempered, rather irritable, very energetic, impulsive, almost fiercely enthusiastic; easily carried away by an idea; very vain especially of newspaper notice, public recognition, and even personal appearance as photo; inclined to magnify his office; very sensitive to interference by colleagues; jealous of new enterprises undertaken by them that had not occurred to him, and well satisfied if such undertakings miscarry; determined to 'get in' if success is probable. This trait is most marked. Very jealous in social matters and of authority and property rights; sensitive to lack of respect by inferiors; has remarkably graceful way of covering chagrin when worsted.

This last description is quoted as a good example of one kind of jealous temperament; and to bring out the fact that jealousy is not limited to morose and sinister individuals. In fact, so far as our results go, it is not associated with any distinctive type, and may appear in many strange combinations. In our returns there were described 185 'jealous temperaments.' Of these, 88 were also mentioned as being selfish, 87 as irritable, 61 as energetic, 56 as genial, 54 as generous, 43 as melancholic, 26 as sluggish. Irritability, selfishness and energy seem, therefore, to be the most frequent correlates; but not by any means necessary ones. The influence of heredity is often mentioned, but not with sufficient surety and uniformity to warrant any conclusions.[1]

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[1] Dr. G. E. Partridge kindly loaned me some statistical data on temperamental qualities of school children of elementary grade. By underlining a number of adjectives or descriptive phrases, printed on child study outline blanks, teachers were able after a year's study to delineate rather fully the character of the pupils under their charge. Out of a total of 1,094 delineations, only 19 (10 girls, 9 boys, ages 7-16) were reported as jealous. Of these 19, 10 were good natured, 11 disagreeable, 9 sensitive, 6 generous, 12 complaining. The smallness of the number reported as jealous can be accounted for by several reasons, which need not, however, to be here considered.

Madame Anna Grudzinska thinks that poets and artists are very often jealous of rivals, of their fame, riches, wives, and their own inspirations. She cites Julius Slowacki (see his *Letters*), Adam Mickiewicz, and Chopin as examples among Poland's eminent men. Victor Hugo and Molière were known to be jealous, and Heine went so far as to poison a poor parrot of whom his Mathilda was fond. Wharton in his Life of Pope, observes, "Among authors, jealousy and envy are incurable diseases." Finck thinks that men of genius, owing to their high emotionality, are usually very jealous. (16, p. 129.)

Jealousy Distinguished from Related States. Anger and fear are so original, so underived that it is almost impossible to define them psychologically except in terms of themselves. This is not true to the same degree of jealousy; for the state of jealousy, it will be found, is a compound or aggregate, and permits of considerable analysis in terms of other feelings. The present phase of our subject will be approached by a descriptive definition of jealousy and allied emotions; the distinctions made will, however, be logical rather than psychological in purpose.

Rivalry is a broad, protean term used by both biologists and psychologists. For Baldwin, it is a complex having the following ingredients: biological struggle for existence, desire of being a cause, love of power, love of the game itself, self-assertion, imitation.

Competition may be made to apply to rather definite cases under rivalry, when the object striven after is more or less clearly in view. (Century.) Both are species of self-emphasis and assertion of power, which are the prerequisite of jealousy, emulation and envy.

Many and various attempts have been made to define *jealousy:* 'Suspicion or bitter resentment at successful rivalry' (Century). 'Apprehension or suspicion of being outdone by a rival in matters of affection or favor' (Standard). 'Aversion to the winner who carries off the trophy of his superiority' (Martineau). Ribot accepts Descartes's definition, 'Jealousy is a kind of fear related to the desire we have of keeping some possession.' By distinguishing jealousy from allied states, its exact meaning becomes clearer. *Emulation*, for instance, may be taken to signify an expression of love of power and superiority for their own sake, but 'being intrinsically neutral as to time and motive.' It is the pure desire to raise oneself to a superior state, best typified in Wordsworth's Happy Warrior of Delight 'whose high endeavors are an inward light.' *Envy*, 'however, designates a more sinister state which contains the pain of inferiority, irritation and a large amount of ill feeling toward the individual who affects us disagreeably by the mere fact of his superiority.' (23, p. 90.) Jealousy is differentiated from envy by the absence of the consciousness of inferiority. As Martineau puts it, envy is 'the grudging sense of relative inferiority.' Aristotle makes a neat distinction. For him, envy is a feeling of pain, *solely* because *others* are prosperous. Emulation is also a species of pain, but solely because *we* are not so prosperous. The former is vicious, being satisfied with a leveling down; emulation is virtuous, and only satisfied with a leveling up. 'Envy to which the ignoble mind's a slave, is emulation to the learned and the brave.'

What are the opposites of envy and jealousy? Spinoza figured out 'that the nature of man is generally constituted so as to pity those who are in adversity and envy those who are in prosperity.' 'Envy is hatred in so far as it affects a man so that he is sad at the good fortune of another person and is glad when evil happens to him.' 'Compassion is love in so far as it affects a man so that he is glad at the prosperity of another person and is sad when any evil happens to him.'

Horwicz discusses this interesting contrast in a lively section of his *Analysen* entitled *Mit- und Fremdgefühle.* (21, pp. 302-325.) 'In a certain sense,' he thinks, 'it may be said that envy is just as natural for the heart of man as is sympathy,' but he adds, 'it is much more difficult to rejoice with the joyful than to be sad with the sad.' 'There are some natures so noble that they can see another in possession of a good for which they have ardently longed, not only without envy, but with actual *Mitfreude.*' Nevertheless, Horwicz finds so much *Schadenfreude* and *Missgunst* that he is prompted to exclaim, 'Do we really harbor in our breast, beside the good, a bad spirit, a devil beside the angel? Is there a purely disinterested joy in disaster itself? Alas, we cannot entirely absolve human nature from the charge.' *Mitfreude* is an academic word rarely used by Germans. Is this a commentary on human nature? Why is it that our own standard dictionary does not print the highly respectable word *confelicity?*

The contrast between pity and jealousy is recognized in Schopenhauer's philosophy. Jealousy, as an expression of the will to live, is a most sinister evil; while pity, which assuages that will, is the highest virtue. But Nietzsche naturally makes pity the thing most despicable. There is a proverb, Pity cureth Envy.

By way of summary, then, we may say that envy arises in a mere situation of inequality (not rivalry), and has no reference to the affection or favor of a third party, no sense of personal injury, but is simply a feeling of inferiority; while jealousy embodies all these things and involves a situation of real rivalry. But, as we shall see, it is a plastic complex, which defies the circumscription of a definition.

Expression of Jealousy. We have already stated by example and otherwise the expressive movements connected with the jealousy of children. The outward manifestations which become extreme enough to be either pathological or criminal are reserved for a later section. We have now to mention in more detail the symptoms associated with the jealousy of generally normal young men and women, as based on their introspections. Numerically these symptoms distributed themselves as follows: Flushing 73; paling 22; chill 16; sweating 26; muscle tension 44; scowling, clenching of fists, compressing of lips, gritting of teeth (one or more or all of these combined), 60. Rush of blood to the periphery and a general or partial tensing of the muscular system were most frequently mentioned. The body assumed an erect position, a rigid attitude, or moved as if in great pain. For one person the chin quivered, the lips became extremely white, the corners of the mouth were drawn. The eyes might be fixed, roll, flash or grow dull. Some would sit very still, others would stamp, crouch, or walk heavily or wring their hands. Several pressed their tongues against the roof of the mouth. Many girls like to seclude themselves or cry bitterly; one young woman claims that no other emotion can make her cry. For others jealousy is too dry a state for tears. Some fall prostrate when they weep. The general complex of symptoms accompanying sulking and obstinacy are frequently present.

Quoting Sutherland, "Dislike, jealousy and hate are, in their physiology, closely analogous to fear and grief. They whiten the skin, reduce the glandular secretions, and depress the vitality; but they have one very distinctive feature. The bodily powers which they restrain are still present though kept in repression. . . . The woman who looks like marble from the deadly gnawing of jealousy will show a deep, dark flush when her rival suddenly comes in sight." (40, p. 289.)

The fact is, as the returns abundantly show, that jealousy being an extremely mutable psychosis, presents many variations between individuals and even in the same individual. Good introspection will discern slight expressive movements in very mild cases, but often enough such movements are small if not imperceptible. Many of the correspondents said they had none whatever. Thus one young woman wrote that her 'envy and jealousy do not express themselves outwardly; they eat inside of me.' Long training and pride may succeed in hiding jealousy from the world's view; but observers of sensibility can discover it even behind a mask. The question, *In the absence of overt acts are you able to detect jealousy? How?* brought many interesting and suggestive replies. Subtle changes in the expression of the countenance were most often mentioned, particularly of the eyes, confirming the proverbial location of envy and jealousy in that organ. As one correspondent put it, 'Jealousy may be detected by a certain expression of the eyes, indescribable but perceptible to observant and sensitive people, an acute, watchful, anxious gaze, averted when it perceives itself watched.' Other indexes mentioned are, the manner of carrying the head, firm set mouth, restless or strained behavior, compression or movements of the lips, slight tightening of the muscles, sighing, elongation of the face, suspicious, pumping questions, over-assumed indifference, inuendo in remarks, chariness of compliments, unguarded words, drooping of corners of mouth or lack of interest shown in listening to another's praise. One young woman perceives jealousy in 'the reserve nature, the flashing eyes, often sulking look. Even where these are not seen there seems to be something in the very air which tells me of the presence of a jealous person.'

Darwin says that his correspondents generally denied that they could recognize jealousy, and thinks that envy and jealousy can hardly be considered to have any characteristic expression, and that the vague and fanciful phrases which poets use in describing these states is a confession that they have no clear outward signs. (9, p. 79, p. 262.) Sir Charles Bell attempts to describe the facial expression of jealousy more fully than Darwin (4, p. 157), but thinks that only poetry like Shakespeare's can truly portray the emotion in the vivid colors of nature, and he considers it a difficult subject for painters.

By the employment of accessories this difficulty is somewhat reduced, and artists do not altogether avoid the theme. I was able to secure two pictures in which it was present, and these I used to test whether an artist can really portray the emotions of envy and jealousy, clearly enough to be correctly interpreted, without the assistance of a suggestive title. The first of the pictures was a colored art supplement by *Geoffroy* from the French Magazine *L'Illustration* (*numéro de Noël*, 1905) entitled *L'Envie*, supposed to represent an envious little girl, intently following behind another little girl of the same age, who was fortunate enough to be the possessor of a coveted bunch of rosy cherries. The second picture was a copy of Mme. Elizabeth Gardner Bourgereau's *The Award of Paris*, which represents a barefoot boy (Paris), age about eight years, presenting an apple to the prettiest of three girls. On the right, and in the background is a girl who is supposed to be jealous. Grasping her skirt with one hand, the little finger of the other hand in her mouth, she looks upon the awarding with askant eyes. The first picture was tried upon 24 subjects, the second on 16. On the first exposure only the envious or jealous girl was shown, the rest of the picture being covered up. On the second exposure the titles only were concealed. The reactions of the subjects were entirely spontaneous, and the results showed little contradicion in the results of the first and second exposure, though the interpretations were more definite when the complete picture was shown. Eighteen out of the 24 guessed *cupidity* (or its equivalent), and 18 out of 24 mentioned *interest*, as being the expression on the face of the *L'Envie* girl. Other judgments were scattered and stated fear, surprise, stealth, etc. Cupidity as meaning strong desire is akin to envy, so we should have to call this a pretty good record. Fifteen out of 16 correctly mentioned jealousy (mostly directly by that name) in the case of the second picture; six used the word envy also, four mentioned shyness, four revenge, two sorrow, ten sulkiness. So, as far as the results of this little test go, we may conclude that envy and jealousy have a characteristic, readable, emotional expression, at least in the face.

Analysis of Jealousy as a Mental State. Several lines of evidence may be adduced to show that jealousy is not a simple mental state. Spinoza spoke of it as 'a vacillation of the mind;' James Martineau as an 'ulterior compound;' Bhagavan Dás makes it a mixture of love and hate; Irons calls it a complex state; Ribot, a 'binary compound derived from the composition by mixture of heterogeneous and divergent elements.' As not infrequently happens, the poets have already furnished a better characterization than any psychologist can provide. Shakespeare sums it all up, according to Finck, in his two lines,

> 'But, O what damned minutes tells he o'er
> Who dotes, yet doubts, suspects, yet strongly loves.

W. Collins in his Ode to Passion, it seems to me, has done as well:

> 'Thy numbers, Jealousy, to nought were fixed:
>   Sad proof of thy distressful state;
> Of differing themes the veering song was mixed;
>   And now it courted Love, now raving called on Hate.'

Our returns abundantly bear out this conception of jealousy, and give us some idea of what are the differing themes. One hundred and twenty-two persons in their introspective accounts mentioned *anger*, as one of the elements. This heads the list. Eighty-four mention *self-pity;* 32, *grief*, sadness, sorrow, melancholy, dejection, despondency; 60, *mortification;* 39, *fear* and anxiety; 55, *hatred*, revengeful thoughts, desire to injure, rancor; 21, *sulking*, impulse to flee from the irritating situation, desire to seclude self and to cry; 21 mention other feelings not falling under the above heads. In one given individual, whatever brand of jealousy he may have, the complex is even for him a veering thing, with now this element now that element prominent. And between individuals the permutations are many. Anger, self-pity and grief constitute the most usual combination, and the resultant jealousy falls into the sthenic or asthenic type, according as the first or last element is predominant. The two grand divisions are 'the jealousy which suffers and the jealousy which is angry,' but either anger or grief may be lacking in any given state.

We shall not push our analysis until we have cited some concrete descriptions, by persons who themselves have undergone the experience of jealousy.

F 22. Jealousy usually arouses in me a feeling of self-pity, anxiety or fear, rarely any of the other more violent emotions. The attack may be sudden or it may be the reaction from some previous strong feeling. The sudden attack comes and goes quickly. My hands always grow cold. A sudden chilly sensation of the spinal column has once in a while accompanied the attack.

F 22. I simply felt a heavy, sinking feeling all through me; hated everybody, and was very nervous; often sulked.

M 23. I felt exceedingly pained at being surpassed in any kind of play and also in love. Toward a rival in love, I felt that he was cruel to all, unfit for his high position. I killed him often and often committed suicide.

F 20. I can scarcely describe how I feel. I get desperately 'blue,' also feel pity for my miserable self, and these are about the only times that I ever cry; I just want to go off somewhere and hide. The attack comes in a flash; merely an action or word on the part of the person may bring it on, and make me miserable for a week and often much longer. Once in its throes, self-control is of no avail; it simply has to wear off of its own accord.

F 18. At first I could n't cry. I just shook with sobs and then I cried as though my heart would break, fell on my knees and began to pray.

F 18. When I am jealous my muscles feel stiff, my heart beats fast and I feel as though I could not draw my breath. Sometimes it seems as though my heart really aches.

F 19. The attack comes on suddenly; when by some word or act I am led to think I am not first in the third party's affection, a pang, a sort of darting pain, goes through my heart, my face clouds over, my lips compress, a chill, stony feeling comes over me, and I pity myself. If I have anything in my hands I clench it tightly. The pain darts through in a second and then I try to shake off the resulting numb feeling. The spell lasts about five minutes; after the spell I am depressed.

F 22. When I am jealous there seems to be something griping my heart. That is all. I feel no anger, no desire to injure, just a feeling as if my heart would break. There is a lump in my throat. I brood, but never give signs of my jealousy outwardly, and I confess that I can't very well drive away the feeling.

F 19. I feel very angry. My face gets very red. I can almost feel my blood boil; by this I mean I have a queer sensation in my body. I was grieved and also felt fear. I felt as though I could tear my rival to pieces.

M 31. Attack is never acute or spasmodic as far as to bodily manifestations, but I have often had a sudden rush of the feeling which caused almost a sinking at the pit of the stomach or tendency to nausea, with flushing. Though often aroused suddenly by trivial circumstances and suspicions, it is rather a prominence on a regular plateau of the chronic feeling entertained toward some rival.

F—. Three times I have suffered very acutely through jealousy toward girls or the women who were favored by the youths or man I loved. My earliest experience came at about 16 or 17, and my feelings were revealed by crying, melancholy, intense longing for dark eyes, graceful figure, and all the superior physical attractions possessed by those I was jealous of. Many physical sensations, as a feeling of a heavy hand grasping the heart and producing actual pain; depression; loss of appetite; loss of sleep; restlessness; desire to walk far away and alone; brooding over what might have been had I only possessed beauty or superior vivacity,—these all accompanied every attack of 'this green-eyed monster.'

F 28. At the first instant I feel a desire to injure, a cold stiffening of my tongue, a cold creeping at my heart. I never can say a word or I say the wrong thing. Am angry, grieved. I feel as if paralyzed. After such an attack I am quite undone, sick, tired, and dissatisfied with myself. I try to exercise self-control but it helps little.

F 31. I felt a nasty, disagreeable pang in my heart. I could cry for hours, lying in a secluded garden corner, or in my room, thinking of all sorts of things, suicide, murder, flight. This feeling vanished, but even when the man was no more loved I felt the same pang when I saw him with another.

M 24. I feel very unhappy sometimes, especially as jealousy makes me lose confidence in those nearest to me. I would be very happy if science could help, it is my great wish and therefore I will be frank. My outbreaks are uncontrollable, I make a row, a scene, and often have had duels. I know I am a brute. I never allow any one to approach the woman I love or court, even if it is a passing flame. I cannot bear to see another man near her. I would kill him. I feel hot, flush, my eyes flash, — it is a most miserable feeling.

The characterization of envy and jealousy in proverbs and poetry, give further insight into the psychology of the subject. The emaciating aspect of envy is most often seized upon in these references: 'Rust consumes iron, envy consumes itself.' 'An envious man waxes lean with the fatness of his neighbors.' 'As a moth gnaws a garment, so does envy consume a man.' 'Base envy withers at another's joy.' 'Envy is the worst disease.' 'Envy is the dyspepsia of the mind.' (Punch.) 'Nothing can allay the rage of biting envy.' 'Wrath is cruel and anger is outrageous, but who is able to stand before envy?' 'The envious man's face grows sharp and his eyes big.' 'Envy pines at good possessed.' (Cowper.) 'A sound heart is the life of the flesh; but envy, the rottenness of the bones.' (Bible.)

Shakespeare variously calls envy, black, devil, sharp, monster; and speaks of 'Lean-faced Envy in her loathsome cave,' and of 'the brinish bowels of some envious surge.' For Pope 'hateful envy' 'howls,' is 'pale' and looks 'wan,' and 'Listening Envy drops her snakes.' Perhaps the most exquisite and masterful literary study of envy is found in the dramatic monologue by Robert Browning, entitled, 'Soliloquy of the Spanish Cloister.' The first stanza may be quoted:

> "Gr-r-r—there you go, my heart's abhorrence!
>   Water your damned flower pots, do!
> If hate killed men, Brother Lawrence,
>   God's blood, would not mine kill you!
> What, your myrtle bush needs trimming?
>   Oh, that rose has prior claims—
> Needs its leaden vase filled brimming?
>   Hell dry you up with its flames!"

If Envy carries snakes, Jealousy is itself a monster with green eyes. In Japan it is proverbially associated with a horned dragon, and the literal translation of the Japanese phrase 'to be jealous' is 'to grow a horn.' An African proverb says, 'A jealous woman has no flesh upon her breast, for however much she may breed upon jealousy, she will always be hungry.' 'Jealousy is worse than witchcraft,' says a European proverb. 'Jealousy is the greatest evil.' 'The jealous man spreads his bed with stinging nettles and then sleeps on it.' Shakespeare also calls jealousy a mad devil, and thinks 'The venom clamors of a jealous woman poisons more deadly than a mad dog's tooth.' The Biblical description again, is vivid: 'Love is strong as death; jealousy as cruel as the grave; the coals thereof are coals of fire, which hath a most vehement flame.'

It is the plurality and diversity of the elements in the jealousy psychosis, combined with their hyper-personal character, that makes it so cruel, so poignant. Bhagaven Dás is of this opinion: 'In its intenser forms, connected with sex-love, where the Love . . . is the greatest, the Hate is naturally at its worst; the consequence is that Jealousy is an emotion which may be said to disturb the mind of the human being, sway it, tear it in two more powerfully than any other emotion.' (10, p. 83.) James observes that the strain on actors playing the part of the jealous Moor in Othello is exceedingly severe. 'Coleman was always physically prostrate after the play and could not get a pigment which would stay on his face.' (24, Vol. 2, p. 465.)

> But through the heart
> Should Jealousy its venom once diffuse
> 'T is then delightful misery no more,
> But agony unmixed, incessant gall,
> Corroding every thought and blasting all
> Love's paradise.

It has been named the 'King of Torments,' and Spenser says, 'Of all the passions of the mind thou vilest art.' Finck is probably right when he calls it, 'The keenest agony known to mankind.'

Genetic development and scope of jealousy. Much of this peculiar anguish is due to the fact that jealousy, at least in its intenser forms, is the acutest hyperself feeling, the keenest self-consciousness experience, which falls to the lot of man. It is pre-eminently an anti-social, self-regarding emotion. In tracing its genesis, we should, therefore, go back to the most primitive feelings of self in the evolutional scale. "In a certain sense," according to J. W. L. Jones, "self-consciousness is coeval with consciousness itself." (25, p. 38.) The lower animals cannot, of course, be credited with a self-consciousness so highly developed as to be conscious of 'the subject whose activity is the subject's object;' but for our purposes a broader conception than this will be serviceable. We shall consider all feelings connected with the instinct of *self*-preservation and *self*-aggrandizement, as either implicitly or explicitly representing a self-feeling. The very definitions of the most primary and irreducible animal feelings like anger and fear, bring in this ascription of a self. Anger is called *self*-aggressive; fear, *self*-defensive. Thus Stanley says, "In fear there is an elimination of oneself from the injury, in anger elimination of the injury from oneself."

In the human self-feeling, according to Ribot, the primary fact is "the feeling . . of personal strength or weakness with the tendency to action or arrest of action." Now it is within the bounds of caution to attribute to all conscious animals a corresponding self-feeling, meaning thereby the halo of pleasure-pain feeling which attaches to intra-organic functioning and adjustment to environment. The base-line of the self-feeling will be physical comfort and discomfort, but its exact nature must be left to individual conjecture.

The first rudimentary emotions to heighten this self-feeling are waves of fear and anger. When these are unblended, undifferentiated, we have nothing, as yet, which may legitimately be called jealousy. But when by reason of more and more complex rivalry situations, which higher organisms have to meet, these two feelings oscillate, conflict or combine with other elements, there may result those peculiar states of antagonism and discomfiture, which, because of their peculiarity, should be called jealousy. The creature becomes jealous when the domain of self is trespassed upon by a rival, or threatens to be so trespassed. The self-sufficiency, complacency, security of some well protected, stolid beasts, suggest anything but jealousy; and their serenity is surely farther removed from such a state than the chronic watchfulness and trepidation of so many preyed-upon fauna.

Whatever may be the character of the sense of self in the higher animals, certain it is, that they act as though their homes, their feeding grounds, their eggs, their cubs, provender and mates were very parts and parcels of themselves. Galton's brilliant description of the sociability of the South African ox forcibly strikes us as being a parallel of what happens in the human family when a man suddenly finds the cherished domain of self sundered by the triumph of a rival lover or business competitor. "An ox when separated from his herd exhibits every sign of mental agony, his glance is restless and anxious and is turned in succession to different quarters; his movements are hurried and agitated and he becomes a prey to the extremest terror, . . . (he) cannot endure even a momentary separation from his herd. He strives with all his might and main to get back, and when he succeeds he plunges into the middle to bathe his whole body with the comfort of closest companionship." Who will deny that this bovine behavior closely resembles the frenzy of human jealousy, and indicates the capacity of even a quadruped to feel shrinkage of self and to take comfort in the restoration of self? The psychology of animal jealousy, then, reduces itself to this: a sense of self highly enough developed to feel imminent or accomplished deprivation in situations of rivalry.

Now in children, as in animals, we should expect the first forms of jealousy to be very rudimentary. If, as seems to be the case, the first manifestation of the proprietary instinct is toward the maternal breast, we may look there for our earliest human jealousy. So far as the infantile sense of self is concerned, it consists in the swaddling period, of bodily feelings of comfort or discomfort, including everything external to the body which becomes connected with its physical welfare, such as the breast, the nurse's face, the father's shining spectacles, the lullaby, etc. These alterior factors are probably earlier and more intimately parcel of the dawning feeling of self than the infant's own hands and feet. At any rate it is true, as Baldwin says, "To be separated from his mother is to lose part of himself, as much so as to be separated from a hand or foot."

It is evident that young infants have a sense of self sufficiently real, to be able to feel the pangs of deprivation, beyond mere organic hunger. At the earliest stage it is solely a sensitiveness to dispossession of things material, which minister to the instincts of food and play, and for several years lusty infantile fights are waged, on account of a jealous regard for these things. But surprisingly early we have the dawn of something which contains the promise of higher forms of jealousy. This is the social sense. The following is taken from the manuscript notes of Dr. T. L. Smith, on the Japanese baby, Kiku. "Her social sense appears to have been an early development, for Dr. C. A. Osborne reports that she cried at being left alone and showed signs of content at having some one near while in the hospital (age three weeks). The first incident, however, which seems to show a distinct element, is recorded in the 16th week, when she was very much interested in a rattle for about ten minutes, but every few minutes turned away from it, looked up into my face with a smile, and then turned back to the rattle. This suggests a seeking for sympathy, though probably in a very rudimentary form." Since then Dr. Smith has noticed a fretting cry, not of bodily discomfort, but of emotional distress, when Kiku's mother would suddenly leave the room or would attend to the typewriter instead of to the baby. Is Kiku jealous of the typewriter? The very suggestion of the question shows that we are dealing here with something which may develop into true jealousy as soon as the sense of self becomes sufficiently ejective.

Miss Shinn noted the same fact in her niece at about the same age. "It was about ten days before the end of the month (sixth month) that she first showed a decided *emotional dependence* on her mother. She had been separated from her for some time (by a tedious dentist's engagement), had become hungry and sleepy and had been frightened by an abrupt stranger. At last she settled into a pitiful, steady crying, stopping at every angle in the corridor where I walked with her, and watching eagerly till it was turned, then breaking out anew when her mother did not prove to be around the corner. This tragic experience left a much deeper mark than the physical woes, and for some days the baby watched her mother rather anxiously as if she feared she might lose her again unless she kept her eyes constantly on her." (37, p. 180.) The baby's emotional dependence was a sign of her capacity to fear and to feel the deprivation of something that ministers to self,—a capacity which is at the bottom of all jealousy, animal or human, material or affectional.

Paola Lombroso's explanation of infantile jealousy, it seems to me, is in harmony with the general interpretation of jealousy here offered. 'The fundamental law of child psychology which co-ordinates and explains the facts of child life is the strong sense of *self*-conservation and *self*-protection, the jealous care with which the child seeks to preserve himself from everything which can hinder or impede even in the slightest degree his development.' 'The child loves merely because of immediate benefit and pleasure conferred upon him.' Lombroso cites as an example a very good, intelligent child who all at once became fiercely jealous, when having broken his leg he had to stay in bed more than a month, and could no longer endure that his little sister should come into the room, to take his mother's attention in the slightest degree. Later this child in giving an account of this jealousy which was short lived, said, "I love Lia now, but when I was ill I did not love her."—'In this case is seen in schematic form, what jealousy really is,' thinks Lombroso; 'a child becomes jealous, when he is weaker, when there is awakened in him a more imperative need of being supreme in affection and gathering to himself all the benefit.' (30, ch. 1.)

Without attempting to trace in detail the dialectic of the developing personal self from infancy on, it may be said that by impingement upon a social environment, the sense of self deepens and expands, becomes more of a socius; and the nature of the jealousy experience grows *pari passu* complex. Jealousy, as one correspondent stated the case, is 'an important phase of the divine spirit of selfishness, which is identical with the instinct of self-preservation.' It may also be subsumed under the protean conceptions of property and of pride. It is the instinct of appropriation embarrassed by a rival, the impulse of domination obstructed, the pleasure of triumph denied (or threatened): it is thwarted pride. It constitutes the substratum of a multitude of childhood foibles, adult littlenesses and exactions, and is a motive in many situations, where we do not ordinarily consider that it exists at all. Many children's lies, I should say, spring from a tendency to level down inequalities, to dominate in spite of everything, and are crude expedients whereby the child saves himself from the pangs of inferiority or self-insufficiency. Dr. Steinmetz's theory of revenge makes it a desire to enhance self-feeeling, and many of the uncanny, malicious acts of children are prompted by such a revengeful jealousy. Thus we have the getting-even, the paying-back psychoses, the calling names, making faces, the don't-speak snubbing, so plainly prevalent among children,—and grown ups. Calling a fortunate rival a scamp, or making some false accusations concerning his victory, repaying slight by another slight, establishes something of an equalization, and wheedles self-pride. A nice white apron is a glaring sign of superiority of Mary over Jane; but a daub of mud soon sets matters right for Jane. If we cannot exalt ourselves we humiliate others, and make ourselves worthy by making them feel cheap; the net result is the same: a more non-irritating level. Similarly, adolescents are prone to resort to extreme ruses all for the sake of forestalling the bitter sense of being outdone.

Revenge is exercised for the same general reason that we desire restitution, when we crave forgiveness on the one hand or exact apology on the other. The administration of justice even is, or historically was, motivated in part by jealousy. Tattling is akin to revenge. Tattling, especially on rivals and teacher's 'pets,' is an inveterate tendency among children. It is the counterpart to the gossip phychosis of their elders. Both are colored by feelings of elation, superiority, smugness; the self is flattered and dark hues are painted upon other selves to heighten the contrast. Rumor is a pipe, and one of the winds which blows it is jealousy. Some one has said rather cynically, 'Envy will be a science when it learns the use of the microscope.' Nothing so sharpens eyes and tongues as envy and jealousy. 'Bad eyes see no good;' sharp tongues derrogate and are sarcastic.

Why are gossipers, and many other people, so inordinately stingy with praise? Why do compliments and congratulations so often give a tug at our heartstrings when they are reluctantly released? Because there is a jealous, ever comparing sense of self-conservation, so sensitive that it suffers from the mere comparisons it makes in favor of another self. Why do we take slights so to heart? Why do children take hazardous dares so readily? Why do we resent taunts, and indeed charges of jealousy? Why cannot some natures brook the slightest criticism or disobedience or accept the kindest advice? And what is the psychological reason for our strong aversion to presumptiveness? There is nothing so intrinsically or ethically despicable in mere conceit, to warrant our tingling contempt for it, and our discomfort. Rather, our irritation is unreasoned, absurdly out of proportion to the cause; the reaction is one of instinctive jealousy.

Normally the intensity of all our jealousy should be inversely proportional to the degree of security we feel. But this instinct has such a momentum behind it, that often its intensity proves ridiculously great when measured by the triviality of the exciting event. This is because in the state of nature jealousy was frequently associated with life and death situations, arousing the creatures to their cruelest ferocity; the most ferocious were victorious and left offspring; they were our ancestors. As Othello most significantly said, "Nature would not invest herself in such shadowing passion, without some instruction." Almost every mother is extremely sensitive about any judgment passed upon or treatment shown her children, who are so much parcel of herself. Here the jealousy probably gets an impetus from the many ages of discipline in the prehuman epoch, when every breeding season called out in the female an impulse to watch and to protect. The most pitiful discrepancies between stimulus and reaction, however, come out in the pathological sexual cases, where the victim is 'as jealous as Ford that searched a hollow walnut for his wife's leman.' (Shakespeare.)

As one self contracts, the other self, which is the cause of the shrinkage, enjoys a corresponding expansion and tends to display and show off its power, and our correspondents often confess, that they feel elated, exultant, proud, patronizing, delighted, self-satisfied, domineering, and triumphant, when they realize that another is jealous of them. One calls it 'a wicked feeling of triumph and elation.' Another says, 'I feel a certain glory in the fact. I desire to exercise what power this jealousy may give me to increase the feeling.' Still another writes, 'When I know a person to be jealous of me, I am triumphant and happy. I feel cruel and try to make her more jealous. It makes me feel wicked but it is a delightful sensation.' In pathological cases, such egotism at another's expense is greatly exaggerated, and the paranoiac finds pleasure and fear in the delusion that the whole world is envious or jealous of him.

Instinct and human social organization have conspired to make the self sensitive. That this self, to which we are so often referring, is no metaphysical abstraction, but a psychological reality, most intimately concerned in the jealousy complex, is shown by the fact that *self*-pity is so frequently part of the psychosis. The presence of this self-pity reveals the peculiar subjectivity, which differentiates jealousy from the simpler, more objective emotions. For jealousy in its higher forms is not like anger, a whole-souled *outward* reaction, but is full of schism, conflict, and introspection. The soul, as it were, splits, and by a miracle both cruel and comforting, subjects itself to its own examination, puts itself upon a rack, gloats over its own sufferings, partakes in them, and pities them. If there is any glint of luxury in jealousy it is by virtue of the self-pity.

"A man's Self," says James, "is the sum total of all that he can call *his*, not only his body and his psychic powers, but his clothes, his home, his friends, etc." (24, Vol. 1, p. 291.) The more highly organized and extensive this self is, the more scope and depth it gives to jealousy. When a self expands wide enough to include not only family, but race and nation, a man may come to be hateful or jealous toward rival races and nations. "The most peculiar self which one is apt to have," however, says James, "is in the mind of the person one is in love with. The good or bad fortune of this self cause the most intense elation and dejection." Although a miser may feel 'personally annihilated' at the loss of his beloved gold pieces, and though the news of a wrecked fortune may bring a cataclysm to the personality of a business man, the most dramatic and instinctive exhibitions of the 'property psychosis,' of jealousy are in regard to the peculiar kind of self, mentioned by James. Every lover will freely declare that the better and greater half of his self, is that immeasurably precious portion of personal property, the object of his love. On the principle of *no love, no jealousy* (which is a proverb in Italy, France and Germany), the greater the love, the greater will be the jealousy. Let any rival break in upon the harmony of love, and the perturbation will correspond to the power and complexity of the original feeling.

Finck makes jealousy one of the 14 indispensable overtones or ingredients that go to make up modern romantic love. For him, jealousy is a savage Cerebus, the watch dog of Monopolism, which latter is the proprietary ingredient. "A genuine Romeo wants Juliet and nothing but Juliet." "I had rather be a toad, and live upon the vapor of a dungeon," said the jealous Moor, "than to keep a corner in the thing I love, for other's uses." Why this terrific monopoly? Psychologically, because the Loved One is in very truth the Self, and jealous monopoly is but self-conservation. The straits to which intrusion into this most sacred shrine of self will drive the outraged man, literature and life abundantly illustrate. He may kill one, or two, or three persons: his rival, his idol, himself. To kill the first is simple, instinctive, defensive, man-slaughter; to kill his beloved is really dire suicide, but seems to him warranted, and to next wipe out the miserable remnant of self that is then left in him, only completes the suicide. He may kill in mere reflex, brutal rage, or in cold-blooded bitterness; but he may also kill in love, as surely did Porphyria's lover in that beautiful, or at least æsthetic murder, described by Browning.—Her yellow, golden hair "I wound, three times her little throat around, and strangled her. No pains felt she; I am quite sure she felt no pain. As a shut bud that holds a bee, I warily opened her lids: again laughed the blue eyes without a stain. And I untightened next the tress about her neck; her cheek once more blushed beneath my burning kiss."—Well may the poet ask,

> How comes this gentle concord in the world,
> That hatred is so far from jealousy?

At the lowest end of the genetic scale is the animal rage of sexual rivalry, at the highest end, an angerless jealousy, all motivated and swallowed up in love,—"a kind of godly jealousy, which I beseech you, call a virtuous sin."

III. THE SPECIAL ASPECTS OF JEALOUSY.

PATHOLOGY OF JEALOUSY. This is a painfully interesting chapter and has received the lion's share of scientific treatment. Some may be disposed to argue that *all* jealousy is pathological, like the Frenchman who said, "*La jalousie est la soeur de l'amour, comme le diable, est le frêre des anges.*" Indeed, Ribot quotes a contemporary writer who defines jealousy as "a *morbid* fear passing from inert stupidity to active or passive rage." Mantegazza called it "a constitutional psychological malady." Burton styled it "a bastard branch of love melancholy." (7, p. 626.) But granting that there is a normal jealousy, it will still be difficult to determine where the same ends and where the abnormal begins. Moreau makes five gradations of intensity from the feeble jealousy to exaggerated. (31) Imbert distinguishes three degrees: *l'obsession*, *l'idée fixe* and *le delire de la jalousie*; but he admits there are so many intermediate degrees that the distinctions are hard to maintain. Imbert also differentiates between idiopathic and symptomatic forms (22). Nosologically, Fere and Dorez suggest that morbid jealousy be classed among the phobias (13). Villers, however, thinks it should be ranged in the great group of delusions of persecution (43). He would, accordingly, make all cases of the disease varieties either of *paranoia alcholia* or *paranoia sexualis*.

Villers, who has made one of the most comprehensive studies of the subject, lists the causes for the affection in the following order: psychic degeneration, alcoholism, hysteria, neurasthenia, troubles with the female genital functions (especially at the climacteric), cerebral traumatisms, senility and cocainism. Almost all cases are engendered by alcoholism. Krafft-Ebing observed morbid jealousy in 80 per cent. of alcoholics still capable of sexual life (28); but the examination of many hundreds of paranoid patients without alcoholic history, revealed not a single case with delusions of jealousy. Excessive inebriety, though at first producing hyperæsthesia, finally results in atony of the genital organs and this diminished sexual ardor seems to form the foundation for the conception of jealousy and its characteristic obscene and erotic accompaniments (6).

Dorez distinguishes between the active and passive cases. The former are represented by the excited, maniacal *persécutes-persécuteurs*, and tend to murder. The latter, which are much rarer, are marked by reserve, melancholia, hallucinations, and tend to suicide. (13) Stefanowski makes two grand divisions: (a) hyperæsthesia of jealousy, and (b) anæsthesia of jealousy. The first class is the more numerous. Everything that "the insane person sees, hears, feels, the perfume of a handkerchief, a look, inspection of undergarments—all furnish traces of an odious infidelity." The unfortunate victim goes to the most pitiable and unwarranted extremes, bathes letters in chemical solutions to discover lines written in sympathetic ink, makes microscopic investigations, "ceaselessly plagues his family with his complaints, reproaches, his despair," and often in a delirium of drink, suspicion and rage, he kills. Sometimes the jealousy is retrospective and is based on circumstances which happened and were forgiven years before. Stefanowski reports such a case in a young Russian, naturally genial and generous, who goaded his wife to confess and reconfess a former faithlessness, and finally beat her to death with a cowhide. (39, p. 387.)

In the anæsthesic class fall the cases no less sad, though more despicable, of the blackmailer who profits by the prostitution of his wife, of the man with such excessive coldness that he divides amours with a rival. Though fierce jealousy is found even among prostitutes and *souteneurs*, society with them tends to extinguish jealousy. Here we have the other side of the picture, which is instinctively repellant, and leads one to recognize that there is a normal jealousy which lies somewhere between the anæsthesic and hyperæsthesic forms.

CRIMINOLOGY OF JEALOUSY. Should the murders prompted by jealousy be called criminal, pathological, or neither? Alienists appreciate both the difficulty and great forensic importance of this question. The public pardons no other kind of manslaughter more readily, and in France, at least, the tendency has been to let such murderers go free. Thus, in one case, a married woman killed her rival six months after the discovery of her husband's adultery, but the court refused to punish. In another case a man who suffered horribly from the tortures of jealousy rose one morning covered with cold sweat, and shot his unfaithful wife. "The medico-legal report declared him irresponsible on the ground that he had obeyed an irresistible impulse which had annihilated his will." (39.) Marc makes the general statement that "Jealousy nearly always decreases the criminality of the deeds which it prompts."

Dr. Hans Gross in his *Kriminal-Psychologie* says that envy is more irreconcilable and by far more universal than other forms of hate, and that it is impossible to overestimate its danger. "Through no other passion, perhaps, are so many lives endangered and destroyed, so many undertakings thwarted, so much that is worthy made impossible, so countless many persons misjudged." (17, p. 555.)

Important as envy and jealousy are as causes of crime, I am informed through a letter from expert special agent, John Koren, of the bureau of census, division of Vital Statistics, that "The criminal statistics published by the goverment yield absolutely no information of value on this subject, nor can anything be found in institutional reports." One fugitive reference states that in the year 1874 jealousy caused the suicide of twenty men and six women in France. (13, p. 50.) Morselli says that for a period of ten years in Italy, comprising 10,347 suicides, jealousy caused nine out of every one thousand with women, and six out of every thousand with men. (43, p. 150.) The present writer secured from a clipping-bureau, newspaper accounts of 'criminal jealousy,' covering all the United-States for 24 days, from May 10, to June 4, 1906. Although these data are undoubtedly incomplete, they are suggestive. Eleven cases of assault not issuing in immediate death were reported, including six cases of shooting, one of stabbing, two razor attacks, and two cases of acid throwing. In addition, there were for this period, seven cases of murder, and seven of suicide.

Envy and jealousy also constitute a prominent factor in juvenile crime. Dorez and Moreau cite several cases in young children. One boy of 10, jealous of the caresses which his mother seemed to lavish on his little brother, 6 years old, cut his throat with a razor. Another boy of 12 in a violent attack of jealousy strangled his little sister, still in the cradle, by forcing a candle into her throat and then filling her mouth and nostrils with hot cinders. In our study of the actions of normal children prompted by jealousy, we often came across deeds verging on criminality. It would seem that this passion merits more attention than it has received, from students of crime.

The question of *sex differences* in criminal and normal jealousy is very complex and delicate. The weight of quotable (male) authority is to the effect that women are more susceptible to jealousy. Confucius said, "The five worst maladies that afflict the female mind are indocility, discontent, slander, jealousy and silliness. Without doubt, these five maladies infest seven or eight out of every ten women, and it is from these that arises the inferiority of women to men." Confucius also made jealousy one of the seven just grounds on which a woman may be divorced. (18, p. 127.) Among the Japanese, it is considered to be an effeminate and feminine trait, and it is one of the first precepts to girls not to yield to it. Weininger says, "Apparently all women are jealous." Heinrich Schurtz finds that woman has less capacity and inclination for social organization than man, and attributes it to the mistrust and ill will which they have for their own sex, and their propensity to gossip. (36, p. 17.) One interesting sex difference has been noted in marital jealousy: a man does not abhor his rival as much as a woman does hers. Marro found the instincts of envy and jealousy were more frequent in young women than in young men, in the ratio of 17 to 1½. But, as Dr. Hall observes, "If it be true, that, as in matters of the heart in general, women are more susceptible to this passion than men, it may well be doubted whether, if it be broadly interpreted, man in his own sphere is not as liable to it." (19, Vol. 1, p. 357.)

PEDAGOGY OF JEALOUSY. The question of sexual jealousy will not be taken up in this connection, for it has been sufficiently alluded to elsewhere. Granted a certain ideal of the family, a proper amount of this passion is most desirable in both romantic and conjugal love. But, happily, it may in most cases be left to take care of itself, nature having furnished the necessary instruction. Its instinctive character should always be recognized, and the danger is that it may often be too easily called out by too slight occasions.

To make a sweeping statement, however, and to say, as Thorndike does, that, "jealousy and rage, for instance, could be omitted from human life with little loss," is rather unwarranted. Oppenheim makes a similar statement and thinks 'every one will unhesitatingly agree that such an emotion as jealousy, however instinctive, is both useless and vicious, that it cannot possibly cause any good result.' These views cannot be left to stand without qualification.

Abbe de Fénelon in his book on The Education of Girls, says, "Jealousy is more violent in children than one would think. They are sometimes seen to languish and waste away with a secret grief, because others are more beloved or caressed than they. Making them suffer this torment is a cruelty too frequent among mothers, but you should know how to use it in a pressing necessity as a remedy for indolence." Rabelais, Bossuet, Locke, Rollin and the Jesuits, likewise, recommended rivalry as a stimulus in instruction; but the classic protest against this doctrine, the effects of which protest are seen to-day in the auto-emulative system of note-books used in the French schools, was framed by Rousseau, who said, "I prefer a hundred fold that Emile not learn at all, what he learns through jealousy or vanity. I would stimlate . . . without rendering him jealous of any one. He would desire to surpass himself."

Those who, contrary to Rosseau, believe in the spirit of rivalry, are inclined (travelling the easier road) to say, Abolish jealousy by substituting for it noble emulation. But the matter is not so simple. Psychologically, the latter is very much like the former. A correspondent writes, "when the perceptive organs apprise the brain centres of a series of facts that the brain centres interpret as evidence of a condition of rivalry, that acts as a stimulus to excite the impulses of antagonism. The finished product is jealousy. In its mild form, we call it emulation and try hard to define it as something different from jealousy. So far as my experience goes, I am convinced that emulation is only a lesser degree of the same feeling that prompts one to murder his rival. They are both aroused by the same set of mental operations, and the mild form often becomes more intense, revealing its true nature." As Beattie says in his Moral Science, "Let the man who thinks he is actuated by generous emulation only, and wishes to know whether there be anything of envy in the case, examine his own heart."

G. E. Dawson thinks that the "sinister passions of anger, jealousy, envy, and oppression indicate the inertia of the human soul, and its resistance to radical processes of change." To him envy and jealousy are psychic rudiments, dating originally to a feral utility; and now their complete elimination should be affected by the processes of atrophy and transformation. "The exercise of continual caution in not tempting children to envy and jealousy sums up the pedagogics not only of common sense, but also of evolutionary law. Where function of the immoral diathesis does not occur, reduction must inevitably follow." (11, p. 221.)

The trouble is, we have as yet no system of ethics by which to measure the immorality of this diathesis, and thoughtful teachers and parents are often at a loss how to regard this passion, when manifested in the young. It certainly awakens a great variety of often contradictory 'moral' responses in those who see this trait in others. Two hundred and four persons frankly told how they felt toward a person whom they knew to be jealous of them. Twenty of them admitted indifference; 37 shunned the person because of fear, awe, coldness, or suspicion; 47 expressed contempt, dislike, anger, disrespect; 37 felt exuberant, proud, triumphant; 32 were led to aggravate and torment; 79 felt kindness, pity, or sorrow, and of these only 23 distinctly say that they try to remove the cause. One tried to be friendly, for she considered jealousy the height of misery, another would humble herself, another would put herself in a ludicrous light, and one even confessed to lying in order to allay the pangs of jealousy.

The whole problem is complicated by so many factors, such as age, sex, temperament, frequency, circumstances, etc., that one hesitates to make generalizations. Attacks of jealousy affect one individual in one way, another in a different way. A few say, that by the force of rebuff generous impulses follow experiences of jealousy. Thus one man says, "Many of the most magnanimous things I have ever done have been done to abase or mortify this instinct." Others feel morose, fretful, cross, mean; some mention physical fatigue, exhaustion, nervousness, loss of appetite; while a few feel relieved. The majority (76 out of 120) say they feel ashamed, dispirited, blue, mortified, penitent, discouraged, humiliated. There are few things which incense a person so promptly as imputations of jealousy; it is so distinctly a matter of self-hood, that even in children it should be treated with utmost delicacy, and care should be taken not to add gall to bitterness by formal precepts about jealousy at the unpsychological moment when the soul has already decided of its own accord, on the unloveliness of the experience. Similarly, it does not add to the sweetness of disposition to play on the jealousy of little children; for this is likely to result in hatred toward parent or teacher, and rival. One kind of jealousy (and there are many kinds) should not be irritated, and should be dreaded as much as a most insidious disease. This is the chronic jealousy between sisters and brothers. The infantile variety which is transitory and explosive leaves no marks; but there is a more sinister variety developed in later childhood and adolescence, which may leave ineffaceable scars. Judges of probate courts can testify that if once the spirit of jealousy is allowed to oppose the peaceful settlement of a testament, a canker has been planted which it is almost impossible to kill, and the family ties are sure to be permanently demoralized. Felix-Thomas rightly deplores that we do not have more and truer *Geschwisterliebe* in our homes, and finds the most redoubtable enemy thereof to be juvenile jealousy, which changes confidence to suspicion and sympathy to aversion. (41, p. 158.) Good honest fights between brothers and sisters are not so deleterious; but the vexation of spirit, and sour quarrelsomeness, and querulousness that accompany jealousy are matted weeds which stifle the flower of *Geschwisterliebe*, and often, too, of filial love. The best horticulture or pedagogy in this case is tactful, and preventive. Courtesy and generosity are best inculcated for their own sake, and not in contrast to jealousy. The child is likely to resent direct precept by reason of the very sensitiveness which makes him jealous, and the effective methods are indirect. It should be remembered that children crave attention in a manner and intensity different from adults. Even an infant will hold out a hurt finger to be kissed, and parents ought to be cheerfully willing to assure and reassure and reassure by formal word of mouth, that they love all their children equally well. This practice, as our returns show, is effective, and supplemented by substantial impartiality would save many pangs and produce good results on the family life.

The child and the sensitive adolescent temperament always need the assistance of sympathy and justice from their social environment; but whenever the self is vigorous, and developed enough it tries to take matters into its own hands. For most of those answering the questionnaire, the attacks came on suddenly, and although some confess entire inability to regulate them, many resort to more or less successful devices of control which are pedagogically suggestive. The following may be mentioned (condensed from the original statements): thinking of good qualities of the rival; turning the attention to other things, as reading, playing the piano, singing, joking, having a good cry, putting self in place of the person of whom jealous; saying something nice about or to the person; prayer, saying, 'What's the use?' and dwelling on the ugly side of the passion. There is an element of control and assuagement in the very self-pity which is so often part of the jealousy complex. Sometimes the injured self finds refuge in imagination, by picturing fanciful situations where it is the centre of attraction and applause; or recalling the one or two cases in which it stands first in the affection of another, or in accomplishments. As one college girl puts it, "I use a little common sense, enumerate the good things I have, and as Mrs. Wiggs says, 'I am thankful I have n't got a hair lip.'"

The whole pedagogy of control in cases of undesirable jealousy then, both from the subjective and objective side, is to say, to do, and to call up those things that restore, conserve or comfort the self which is wounded. There is a transitory, lusty kind of jealousy, which is, if anything, a good sign, for it indicates a vigorous sense of self, and the possibility of noble emulative spirit; but there is also a chronic, gnawing, envious kind, which is both physically and mentally unhealthy, which denotes rather a morbid sense of self and develops the opposite of emulation. If in this delicate and complex field, a practical suggestion may be hazarded, it would be this: Develop in your children a robust spirit of self worth; if possible let them have a hobby, a specialty, a pre-eminence in one particular,—not to foster conceit, but to promote a legitimate personality sense. This particular pre-eminence may be a haven of consolation, when buffeted by rivalry one is overcome with the sense of inferiority, and may make it possible for one to say after reflection, 'I am glad to be myself.' When the soul can say this heartily it is free from the corrosion and demoralization of intermittent, chronic envy and jealousy. Such a self need not be non-jealous, but it will be healthily jealous.

ANTHROPOLOGY OF JEALOUSY. From what we have seen of jealousy in animals and young children, we should hardly expect the passion to be absent among primitive peoples. Darwin considers "that almost promiscuous or very loose intercourse was once extremely common throughout the world;" "nevertheless," he adds, "from the strength of the feeling of jealousy all through the animal kingdoms, as well as from the analogy of the lower animals, more particularly those which come nearest to man, I cannot believe that absolutely promiscuous intercourse prevailed in times past, shortly before man attained to his present rank in the zoölogical scale." (8, ch. 18). Maine agrees with Darwin that jealousy could hardly have been dormant in primitive man, and Starcke in his theory of the primitive family combats the idea that marriage ever passed through a general polygamous stage, citing mutual jealousy as an obstacle thereto. After producing numerous instances in which primitive man willingly surrenders his wife to others, Starcke submits that "the rule may be laid down that jealousy was only excited when the man was afraid that he should lose his wife." "There is no race," Sutherland believes, "entirely without sexual jealousy, but in some it is only slightly developed." But he mentions 17 races among whom wives are easily leant or bartered. He also found 50 or 60 races among whom the bridegroom actually prefers that his bride should already have borne a child." (40, p. 131.)

Finck goes farther than any other writer in making primitive peoples strangers to jealousy. "Among some species of birds," he says, "courtship and marriage are infinitely more refined and noble than among the lowest savages." "I assert without fear of contradiction from any one familiar with anthropological literature, that a savage or barbarian, be he Australian, African, American or Asiatic, would laugh at the idea of refusing to exchange one woman for a dozen others equally young and attractive." (15, p. 54.) "The most painstaking research has failed to reveal to me a single Indian tribe in North or South America that showed a capacity for real jealousy; *i. e.*, anguish based on a sense of violated wifely chastity and alienated affection." (15, p. 89.) Feminine jealousy is said to be lacking even in polygamous households like those of the Zulus, the Fulahs, and according to H. Ellis, of the Koreans. Finck, however, mentions that the Patagonian women fight like tigers from jealousy, and that with the Fijis plurality of wives often causes the stronger women to bite off the noses of the weaker. In spite of these fierce manifestations, he thinks primitive female jealousy only skin deep, easily placable, and concludes that "real jealousy, as a matter of fact, is unknown to the lower races, and even the feeling of revenge that passes by that name is commonly so feeble as to be obliterated by compensations of a more or less trifling kind."

Westermarck is probably a safer guide in this question, because he presents his evidence with greater care. He enumerates some eight peoples among whom there appears to be no jealousy among the women, and where in a few cases, like the Equatorial Africans, the women actually support and favor polygamy. (44, p. 495.) But against these instances, he presents an imposing list of over 20 peoples of whom the opposite is true, where the women not only fight, but commit suicide from jealousy (as seen in some American Indians). A foot-note to this list gives 17 additional references to cases of female jealousy, representing a large diversity of primitive peoples. Westermarck derives the strongest argument against the hypothesis of promiscuity "from the psychical nature of man and other mammals." He arrays evidence in regard to some 30 uncivilized peoples, some of a very low grade of culture, who by their laws, customs, and often terrible punishments, show a capacity for jealousy. (44, pp. 117-133.) The males among some Indian tribes resort to the law of battle, and fight for the possession of their women. Surely we cannot deny them, at least, the jealousy of the stag. And as Westermarck observes, "If the hypothesis of an annual pairing time in the infancy of mankind holds good, jealousy must, at that stage, have been a passion of very great intensity."

Wm. Jones, of the American Museum of Natural History, New York, in response to my letter of inquiry writes in regard to the widely distributed Central Algonkin Indians, that (a) the dialects of their language are rich in the expression of envy and jealousy; (b) the children manifest envy and jealousy at an early period and betray it in much the same way as so-called 'civilized children;' (c) men and women quarrel among each other over property in much the same manner as 'civilized people;' there is a great deal of envious gossip; (d) envy and jealousy are reflected in proverbs, folklore and in the body of unrecorded literature of the people and are usually regarded unpleasantly; (e) the nature and extent of jealousy for wife and jealousy for husband are much the same as among 'civilized people.'

Geo. A. Dorsey, of the Field Museum of Natural History, Chicago, writes substantially the same on all these points in respect to the Pawnee Indians: (a) the Pawnee word for jealousy is *kawirao*, meaning 'hurt inside;' (b) often poor Pawnee boys in their jealousy or envy steal the clothes or other objects of boys better to do and throw them away or destroy them; (c) the Indians are notorious gossips, and formerly, the old people claim, quarreled almost constantly regarding property, especially concerning ponies, products of the chase, etc.; (e) the men among the Pawnee are exceedingly jealous of their wives, and quickly resent any attention they might bestow upon other men; the women are also jealous, but less so.

Jas. Mooney, of the Bureau of American Ethnology, writes from an acquaintance with the Cherokee, Kiowa, Comanche, Cheyenne, Arapaho, that they are all "extremely jealous of reputation, position, and ability, and in their marital relations, both men and women. They are not, as a rule, jealous or envious of property possession. The children are equally jealous of attentions, but seldom quarrel about possessions. As compared with us, jealousy is strong, envy or covetousness weak."

Geo. B. Grinnell, editor of Forest and Stream, finds very wide difference in regard to the nature and extent of wife jealousy, and credits the Crows and Arapahoes with but little jealousy, and the Cheyennes with much.

Boaz believes there is no great gulf separating the primitive and the civilized mind, and all the specific evidence just presented favors the view of Darwin and Westermarck, that there was never a time when man was devoid of the powerful feeling of jealousy.

Abundant material might be collected illustrating the rôle of marital jealousy among non-primitive peoples. It "prompted Greek and Oriental to put wife under lock and key, and Chinese to mutilate their wives' feet, and the Japanese to have their wives shave eyebrows and blacken their teeth after marriage." (16, p. 129.) Jealousy also was responsible, according to Westermarck, for the only recently abolished custom in India which demanded that the widow should be burned on the funeral pyre of her husband.

Further light on the prevalence and degree of jealousy among primitive races, must be sought in connection with their conceptions and institutions of property. Here, again, according to Kline and France, "some savage peoples are inferior to animals in that they possess almost nothing, *e. g.*, peoples in the woods of Borneo and the Forest Weddahs of Ceylon, . . . and when primitive man does hold property it is to a large extent in common." The chief form of property for the primitive horde was the hunting-ground, and this had to be jealously defended against competitors, and we may well imagine that roving bands of troglodytes often had to join in death grapple in their rivalry for the same coveted cave. But within the tribe the communistic system of ownership would tend to put a damper on jealousy, which only became strong as the conception of private property developed. "Communism," think Kline and France, "is the best evidence of mental dullness, physical laziness and primitive lethargy;" and imply that progress does not begin till jealous self-assertiveness is wakened (27, p. 440). Rousseau has painted in classic colors, the idyllic, non-rivalrous primitive state, where sinister jealousy never disturbs the calm.

All customs, laws and institutions, primitive or civilized, ancient or modern, if properly ransacked, would beyond doubt furnish material to illustrate the power of the jealousy psychosis, not only in personal, but in social matters. Such an undertaking, of course, is not within the limits of this study, but a few brief examples taken from the field of religion may be offered in passing. "Have the gods envy?" asks one of Shakespeare's characters, and answers, "Ay, ay, ay, ay, 'T is too plain a case." Jealousy was a veritable psychosis coloring all Greek mythology and literature. They and the Romans even had gods or goddesses of envy, rivalry, and jealousy, and for that matter the whole Olympian galaxy who did not bear these names were an envious pack. Zephyrus was jealous of Apollo, Juno of Io, and Jupiter of the whole human race, especially of Prometheus. Jealousy prompted him to one of his most disreputable deeds,—the malicious Pandora box.

Among the Hebrews likewise, in bold and picturesque figure, jealousy is repeatedly ascribed to the great Jehovah. 'He is the husband, Israel, the wife; idolatry and wickedness of every kind are spiritual adultery' (2, p. 553), and often with abominations was He provoked 'whose name is Jealous' (Ex. 34:14). Jehovah is Himself made to say, 'For I, the Lord, thy God, am a jealous God' (Ex. 20:5); 'jealous with great fury' (Zech. 8:2); 'Surely in the fire of my jealousy have I spoken against the residue of the heathen.' In Mediæval Christianity we have one of the strangest outcrops of jealousy in the female mystics. With them the tables were turned: Jehovah is not jealous; but they themselves are jealous of their divine lovers, God and Jesus.

SOCIOLOGY OF JEALOUSY. To what extent has jealousy been a factor, beneficial or otherwise, in shaping social progress? The chief and fundamental unit of society is the family, and no complete consideration of the family can exclude the principle of sexual jealousy. In Sutherland's opinion, "the family is strong in its cohesiveness and distinct in its form only when there is a very decided infusion of sexual jealousy in the national character." (40, p. 130.) Imperfect jealousy among uncivilized communities tends to blur the family lines. 'Its moral mission among highly civilized persons, is to aid in developing the romantic features of love.' Even if the rude savage regards his wife as a piece of property, and even if the origin of the family is to be traced to the proprietary instinct, sexual jealousy at its best is a 'property psychosis,' whose chief function is to resent intrusion, and as such it has been the potent influence in developing chastity, conjugal fidelity, and monogamy. So long as society desires this trinity of virtues to be embodied in the family institution, it is to be hoped that sexual jealousy, at least, will not dwindle.

Jealousy as a sociological factor also raises the whole philosophy of Socialism with its theories of the present and a desirable future society. This philosophy holds that the existing industrial organization rests on the principle of competition between individuals, corporations, and nations; that this principle is an inferior one, and both should and will be displaced by the higher principle of co-operation. John Stuart Mill, writing on The Stationary State, says, "While minds are coarse they require coarse stimuli, and let them have them. I confess I am not charmed with the ideal of life held out by those who think that the normal state of human beings is that of struggling to get on; that the tramping, crushing, elbowing, and treading on each other's heels, which form the existing type of social life, are the most desirable lot of human kind." Whether or not we agree with Mill's view that this competition is only a transitory phase of industrial progress, it is often true, as Le Bon says in his Psychology of Socialism, that present day business "competitors put up with one another because they cannot do otherwise, but the tenderest sentiment they entertain for one another is ferocious jealousy." If the co-operative commonwealth, which Socialism desires, is ever established, it must be by the blending of interests, the encouragement of Kropotkin's instinct of mutual aid, thereby reducing rivalry and lessening the occasion for jealousy. (It is interesting to notice that philosophers with socialistic tendencies, by very temperament, it seems, sometimes favor also the elimination of sexual jealousy, as indicated in the doctrine of free love, which doctrine, however, is by no means an essential tenet or corrolary of modern socialism.)

In a competitive society where jealousy is given free play, its influence is paradoxical, being in two opposite directions, one toward democracy and equality, the other toward exclusiveness and variation. This seems to be an expression of Tarde's two laws of imitation and of opposition. The first tendency is a levelling one; every man considers himself at least as good as the next and wants the same privileges, etc., is jealous if there is inequality. But by his nature he is jealous of equality as well, and wants to be different from the next man. When A buys a piano it must be, if only a little, more stylish than neighbor B's. Fancy dressmakers are said never to make two dresses just alike; their customers will not allow it. Many examples might be cited to show that in social matters, where imitation and rivalry are at work, the tendency of jealousy is both to maintain and to break a level.

This comes out clearly in the operation of *group jealousy*. Our questionnaire returns report some 170 cases of such jealousy, and many of the descriptions instructively show its great social significance. Churches might be expected to be free from this motive, but they are the most frequently mentioned in the returns. The causes given include many thing, from chimes and steeples to the size of congregations. Fault-finding gossip, 'cutting dead,' splitting into factions, and spiteful absence from union services are common manifestations.

Commercial jealousy is a species by itself, which, owing to the complexity and militancy of our industrial life, has developed many virulent and peculiar forms, from the backbiting of corner grocers, to the drastic Machiavellianism of the great syndicates. Towns, juvenile societies, men's and ladies' clubs, girls' cliques, boys' gangs, all may develop, when the rivalry is sufficiently keen, a spirit of jealousy expressing itself in taunts, gossip, libel, secessions, exclusiveness, imitativeness. Jealousy between families expresses itself in the same general ways. Tacitus in describing the ferocity between two hostile German tribes makes a rich remark that "they hated each other like neighbors!" The 'neighborliness' of civilized families is tinctured with enough jealousy to make the simile ring true. Illustrations are hardly necessary.

Group jealousy is explainable in terms of the Self, just as is individualistic jealousy. The personal self has simply widened: "the socius, the common self of the group, comes in to drive out the narrower ego of his relatively private life within the group." By the force of imitation and tradition this peculiar social self becomes as sensitive as the private ego; sometimes it seems to be more sensitive, and a man comparatively dull or neutral in private life may exhibit intense spirit, often of a jealous kind, when his group self is irritated.

Now if churches, clubs, towns and families, are not too unwieldy to be moved by jealousy, it is probable that the larger organizations, clans, counties, nations, races, may be so moved, and we are reminded of "France and England whose very shores look pale with envy of each other's happiness." In the personal memoirs, which are the best documents for depicting the inner psychological forces which shape historical destinies, we are favored with many glimpses of our friend Jealousy, who in parliaments, diplomatic circles, congresses, official balls, in places public and private, is motivating the actions which contribute to the current of history. Balance of power, spheres of influence, partitioned protectorates, retaliatory tariffs, secessions, dual governments, federalistic schemes, all directly or indirectly bear witness to the existence of the jealousy of states and nations. The mere citation of examples would take too much space; but this brief general reference may serve to indicate that jealousy is important for the social as well as individual life. An adequate philosophy of history or society cannot neglect this vital instinct.

IV. SUMMARY.

1. Animal jealousy bears a close relation to anger and fear, and is a fundamental instinct, appearing in the lowest vertebrates and in the higher invertebrates. It is connected chiefly with feeding, mating and breeding, and serves as a corrective for the purely social instincts, thus protecting the individual as against the group.

2. Being phylogenetically so fundamental, jealousy appears

very early in the human. Its first manifestations are in regard to the maternal breast, but it continues into old age.

3. Jealousy is interpreted as a self-feeling, which depends for its content not only upon instinctive rivalry, but upon the influences of the social environment. Genetically the emotion shows important developments in complexity and refinement, as the sense of self deepens and expands. Childhood jealousy presents marked changes as compared with infancy. Puberty brings a special increment of sensitiveness.

4. The expression of the emotion exhibits changes, correspondingly. The infantile expressive movements are mainly overt, aggressive and highly instinctive in character. At adolescence, depressive, melancholic symptoms become frequent; they may be either subtle or severe.

5. An analysis of jealousy as a mental state proves it to be peculiarly complex and variable, and perhaps the most painful of all emotions. The commonest constituents are anger, grief and self-pity. The painfulness is due to the intense subjectivity of the psychosis, to the obstruction of impulses of pride and appropriation, to the disorganization of profoundly egocentric, and highly systematized ideas.

6. The pathology and criminology of jealousy reveal the tremendous practical importance of the instinct and the desirability of its further study especially in its non-morbid aspects.

7. The best pedagogy of jealousy for children and youth is probably indirect and preventive, and consists in the cultivation of a healthy personality sense.

8. Anthropology and the history of society demonstrate the significance of sexual jealousy for the family institution, for chastity, monogamy and conjugal fidelity.

9. The scope of jealousy is shown to be much wider than is ordinarily recognized. Jealousy is at the basis of many attitudes which the individual takes toward his fellows; it colors social customs and institutions; it motivates group action.

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MEMORY FOR LIFTED WEIGHTS.

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The object of the investigation was two-fold; on the one hand to study the influence of the interval upon the accuracy and quickness of recognition, and on the other to determine the mental processes involved in comparison and recognition. Lifted weights were used because they promised a domain where memorial images are very weak and would therefore shed light upon the place of the memorial image in recognition or comparison.

The results have, in a measure, at least, justified the expectation. As the historical side of the question has been pretty thoroughly covered in various articles, it seems advisable to omit in this connection further discussion of that, and to proceed at once to a description of the experiments, and an analysis of the data thus obtained. The experiments reported in this paper were begun in the fall of 1903 and continued without interruption until the middle of June of the following year.

EXPERIMENTAL.

The reaction times of the subjects were taken by means of a Hipp chronoscope, giving, on the average, readings which were correct to within 1.9σ. The subject was seated at a desk of convenient height, so that the right forearm rested comfortably upon it. The back of the hand rested upon a reaction key, which with a lip-key was used in the earlier experiments; but as it was found to be in many ways inconvenient, a Cattell speech key as modified by Walt was substituted and gave complete satisfaction. When the reagent lifted his hand from the reacting key, the circuit was completed through the chronoscope; and when he announced his judgment by speaking into the tube, the vibration of the disk broke the circuit, and in this way the total reaction-time was registered. The amount of flexion of the arm was regulated by a padded bar placed at a suitable height above the table, so that in lifting the weight, the wrist touched the bar.

A determination of the time taken in lifting the weights was made from time to time to see how constant its value was in the case of the different reagents: the mean variations range from 3% to 8% for the different subjects. When we compare these figures with the mean variations for the recognition-times reported in the tables, it is evident that the control of lifting was relatively rigorous.

A series of eight Jastrow weights were used, ranging from a few grammes (20) up to a heavy weight of 600 grammes. Two of these, one of 250 gms. and the other of 225 gms. were kept constant thoughout the experiment, and furnished the weight intervals given in the tables. The weights were identical in shape and size consisting of hollow hard rubber cylinders, 4 inches long and 2 inches in diameter, which could be filled with a variable amount of shot kept from rattling by pads. The weights were kept out sight of the reagents by means of small screens, so that they remained in ignorance of the number of weights used. The "now" was given two seconds before the weight was placed by the experimenter in the hands of the reagent for lifting, and the time between the lifting of the norm and the stimulus of comparison was regulated by a stop-watch graduated to one-fifth of a second. The first weight lifted was taken as the norm, and the second weight as the stimulus, which was reported by the reagent to be "lighter," "equal," or "heavier" than the norm. For the standard intervals, the order of presenting the weights was reversed sometime during the experiment, as is the customary procedure to avoid constant errors. To reduce habituation to a minimum, the weights compared were chosen in irregular order; that is to say, the standard intervals which give the reaction times of the tables were interpolated between several other intervals. Calling the weights A, B, C, D, E, F, G, H, of which D and F, we will suppose, are the standards, we would have something like the following combinations in a typical experiment: B-C, *D-E*, G-H, *D-D*, *F-F*, C-H, *F-D*, *F-F*, etc.

The reagent, at the end of each comparison, gave an introspective account, as best he could, of the process of judgment. With some of the less experienced reagents, these reports were at first rather vague and confused; but soon each developed a terminology of his own that greatly abridged the task of recording the introspections. Out of this grew the classification of judgments used in table III. The reagents reported cases in which the norm and stimulus of comparison, were distinctly placed in a scale of values, or in which only one was thus carefully classified, with little or no reproduction of the sensations occasioned by the lifting of the norm. Here, of course, if any reliance is to be placed upon introspection, the central process is largely one of verbal supplementation. Other cases occurred in which the stimulus of comparison did not seem to have an absolute position assigned to it in the scale of weight values, but yet did have a position relative to the absolute position of the first. Both have been treated together in the tables under the head of absolute judgments. In another class of judgments, along with the verbal supplementation, went a considerable amount of memorial image representation, in which the reagents assigned about equal importance to the two processes in the comparison of norm and stimulus. (It should be noted in passing that although the judgment in lifted weights is primarily directed to strain sensation, other sensations, in particular, visual, are integral parts of the mental activities involved, and with one exception, Mr. Freund, the visual memory of the arm movement was more predominant than the memory of the strain and pressure sensations.) Judgments of this type are reported as "mixed." In other instances, verbal supplementation played an insignificant rôle; the memory image in some form (visual, of arm movement, or motile, of pressure and strain) seemed clear enough to mediate comparison; such are recorded as "direct." One subject (Mr. Wright) furnishes a few judgments, sixteen, in which neither verbal supplementation nor the reproduction of memorial images was noticeable. The writer is inclined to regard these as of a negative character, due to inadequate or hasty introspection on the part of the reagent. They have been given a place as the fourth type of judgment under the denomination of "immediate."

The time intervals chosen for record were 20 secs., 30 secs. 40 secs., 50 secs., 60 secs., 80 secs., 100 secs., 120 secs. At first shorter intervals of 5 secs., and 10 secs. were used; but these were found on trial to be too short to permit the experimenter to perform his share of the work without some confusion and embarrassment. Now and then a longer time interval was interpolated, either in a blank experiment or in one desired for the introspective account. Each subject filled in the time between presentation of norm and stimulus with such mental occupation as he pleased.

Five subjects took part in the investigation: Dr. Pillsbury (P.), Mr. Galloway (G.), Mr. Wright (W.), Mr. Sherman (S.), Mr. Freund (F.) All except the last had had considerable training in laboratory work, the last, one semester's work in an introductory course.

ANALYSIS OF THE TABLES.

A. The Effect of the Memory Interval on the Time of Recognition. In the table below are given the average reaction times of all judgments taken together for the different intervals of time and for the whole eight intervals considered as a unit:

An inspection of the table fails to reveal the existence of a factor whose influence can be expressed in the definite mathematical fashion in which has been presented the time-course of the dissolution of a memorial image. We find for $\triangle = \pm 25$

TABLE I.

Average reaction times (r.t.), for the various memory intervals (t.i.), for the weight difference (Δ) of 25 ± gm and 0 gm, both right and wrong judgments taken together.

t. i.	20s.	30s.	40s.	50s.	60s.	80s.	100s.	120s.	Av.	No.
P.										
$\Delta = \pm 25$ g. r.t.	1.292s	1.276s	1.152s	1.113s	1.189s	1.262s	1.285s	1.276s	1.228s	1067
$\Delta = \pm$ 0 g. r.t.	1.280s	1.223s	1.249	1.201	1.211	1.225	1.312	1.293	1.245	977
G.										2044
$\Delta = \pm 25$ g. r.t.	1.007	1.004	.958	.942	.964	.980	1.013	.996	.983	812
$\Delta = 1$ 0 g. r.t.	1.049	1.050	1.042	1.017	1.025	1.043	1.056	1.083	1.043	901
W.										1713
$\Delta = \pm 25$ g. r.t.	.887	.803	.746	.780	.809	.819	.769	.839	.831	401
$\Delta =$ 0 g.	.945	.762	.915	.797	.884	.853	.966	.938	.894	465
S.										866
$\Delta = \pm 25$ g. r.t.	1.077	.983	.992	.935	.993	1.036	1.059	1.143	1.027	210
$\Delta =$ 0 g. r.t.	1.098	1.083	1.158	.976	1.032	1.254	1.072	1.082	1.190	246
F.										456
$\Delta = \pm 25$ g. r.t.	.702	.848	.850	.881	.908	.926	.683	.818	.838	141
$\Delta =$ 0 g. r.t.	.781	.959	.818	.770	.895	.740	.826	.839	.829	157
										298

gm., that in the case of P., the reaction times decrease gradually from the 1st interval (1.292 s) on to the fourth (1.113 s) and then rise again more or less gradually during the remaining four intervals; in the case of G., essentially the same course is shown. The decline is from 1.007 s in the first to .942 s in the 4th, and the rise during the remaining four; in case of W., there is decline from .887 s in the first to .746 s in the third, with a steady rise to the end, with the exception of a drop to .769 s in the 7th; in case of S., a course somewhat irregular, but with the values higher for the latter intervals than for the earlier and with the minimum of .935 s in the 4th interval; in case of F. a still more irregular course, with a maximum of .926 s in the 6th interval and a minimum of .683 s in the 7th.

For $\Delta = 0$ gm., the general results are much the same. The reaction-time declines for P. from 1.280 s in the first to 1.201 s in the fourth, rising in a fairly gradual way during the last four intervals. G. shows an irregular decline from 1.049 s in the first to 1.017 s in the 4th., with a gradual rise during the remaining. In case of W., there is less regularity in the course of the reaction time for this weight interval than for the other: The minimum of .762 s is reached in the 2nd interval, and the maximum of .915 s in the ~~4th~~ 3rd, and then an irregular rise during the latter intervals. S. shows a gradual decline from 1.098 s in the first to .976 s in the 4th, and then an irregular rise during the latter intervals. F.'s course is irregular, the minimum of .770 s occurring in the 4th interval and the maximum of .959 s in the 2nd. The reaction times are in general higher for the difference △ = 25 ± gm., than for no difference. Taking the average of all the intervals, we have the following: P. 1.228 s for △ = ± 25 gm., and 1.245 s for △ = 0 gm.; G., .983 s and 1.043 s resp.; W., .831 s and .894 s resp.; S., 1.027 s and 1.190 s resp.; F., .838 s and .829 s resp., the single exception to the statement.

The most, therefore, that we are entitled to say is, that there is gradual decline in the reaction time during the first 50 or 60 seconds, and then some increase for longer intervals of time. Fifty to sixty seconds seems to be the most favorable interval for judging the weights so far as the shortness of reaction can be taken to indicate this. This accords with what Prof. Angell found in his experiments on the discrimination of shades of gray for different intervals of time, though he did not extend them beyond 60 secs.[1]

The general statements just made are still further confirmed, on examining the values of the reaction time for right and wrong judgments treated separately, as will be seen by inspecting the following table:

Confining our attention at first to right judgments for △ = ± 25 gm., we find that in case of P. the reaction time drops from 1.154 s in the first interval to 1.039 s in the third and rises in a fairly regular way to 1.189 s in the 8th; in case of G., a slightly irregular fall from .955 s in the 1st to .892 s in the 5th; with a somewhat higher level of values in the last three; in case of W., a decline, fairly regular, from .927 s in the 1st to .670 s in the 5th, and gradual rise during the last three to .729 s in the 8th; for S., an irregular fall from 1.010 s in the 1st to .803 s in the 4th, with higher values in the latter places; for F., an irregular course, with minima of .682 s in the 1st and .644 s in the 7th, and a maximum of .997 s in the 5th.

The average for wrong judgments for this weight difference shows a rough tendency to follow the same general course as do the right judgments. P's figures rise and fall till the 4th interval when a minimum of 1.177 s is reached, after which there is a fairly regular rise to 1.376 s in the 8th interval. About the same is true of G., who has a minimum of 1.046 s

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[1] Phil. Studien, Vol. XIX, p. 19.

TABLE II.

Reaction Time in seconds for Right and Wrong Judgments. N. standard stimulus; V. stimulus of comparison; r. right and w. wrong judgments.

t. i.	20s.	30s.	40s.	50s.	60s.	80s.	100s.	120s.	Av.	No.
Dr. Pills.										
Δ = ± 25.										
r: V $\lessgtr$ N	1.154s	1.123s	1.039s	1.072s	1.131s	1.150s	1.200s	1.189s	1.135s	545
w: V $\gtrless$ N; r.t.	1.286	1.342	1.240	1.140	1.270	1.366	1.368	1.351	1.298	293
w: V⦀N; r.t.	1.338	1.370	1.295	1.246	1.237	1.350	1.384	1.396	1.311	229
w: Av.; r.t.	1.313	1.354	1.364	1.177	1.260	1.358	1.376	1.376	1.304	522
										1067
Δ = 0										
r: V⦀N; r.t.	1.271	1.182	1.236	1.158	1.247	1.259	1.276	1.293	1.231	311
w: V < N; r.t.	1.293	1.275	1.250	1.207	1.128	1.291	1.327	1.303	1.252	302
w: V > N; r.t.	1.283	1.266	1.269	1.250	1.226	1.134	1.313	1.287	1.249	364
w: Av.; r.t.	1.287	1.252	1.262	1.260	1.168	1.211	1.319	1.293	1.250	666–977
Mr. G.									grand	tot'l 2044
Δ = ± 25										
r: V $\lessgtr$ N; r.t.	.955	.874	.909	.896	.892	.928	.934	.903	.916	481
w: V $\gtrless$ N; r.t.	1.069	1.085	1.046	1.040	1.054	1.070	1.137	1.067	1.074	145
w: V⦀N; r.t.	1.082	1.098	1.063	1.051	1.045	1.077	1.123	1.112	1.083	186
w: Av.; r.t.	1.075	1.092	1.057	1.046	1.047	1.074	1.127	1.089	1.079	331
										812
Δ = 0										
r: V⦀N; r.t.	1.041	1.048	1.037	.991	.983	1.030	1.047	1.057	1.029	309
w: V < N; r.t.	1.070	1.053	1.042	1.029	1.047	1.044	1.057	1.062	1.052	294
w: V > N; r.t.	1.044	1.048	1.054	1.041	1.056	1.052	1.060	1.058	1.051	298
w: Av.; r.t.	1.057	1.050	1.048	1.036	1.052	1.048	1.058	1.068	1.052	592–901
Mr. W.									grand	tot'l 1713
Δ = ± 25.										
r: V $\lessgtr$ N; rt.	.927	.803	.677	.670	.703	.652	.694	.729	.709	230
w: V $\gtrless$ N; rt.	.736	.921		1.307	.877	1.031	.818	1.133	.868	122
w: V⦀N; rt.	.949	.709	.832	.821	.968	1.174	.894	.937	.917	49
w: Av.; rt.	.863	.803	.832	.967	.944	1.042	.819	1.014	.901	171
										401
Δ = 0										
r: r.t.	.892	.648	.763	.578	.741	.698	1.030	.964	.827	212
w: V < N; r.t.	.832	.901	1.183	.944	.988	.847	.994	.898	1.011	174
w: V > N; r.t.	1.970	.696	.539	1.013	.889	.972	.728	.957	.812	79
w: Av.; r.t.	1.060	.815	1.041	.965	.950	1.007	.892	.915	.967	253–465
Mr. S.									grand	total 866
Δ = ± 25.										
r: V $\lessgtr$ N; r.t.	1.010	.933	.969	.803	.660	1.070	1.039	1.016	.956	131
w: Av.; r.t.	1.157	1.126	1.038	1.140	1.085	.992	1.078	1.148	1.118	79
										210
Δ = 0										
r: V⦀N; r.t.	1.165	.688	1.214	1.161	1.154	1.167	1.207	1.118	1.027	97
w: Av.; r.t.	1.015	1.165	1.108	.976	1.314	1.105	1.016	1.221	1.113	149–149
									grand	total 456

TABLE II.—*Continued.*

t. i.	20s.	30s.	40s.	50s.	60s.	80s.	100s.	120s.	Av.	No.
Mr. F.										
= ± 25.	.682	.890	.819	.770	.997	.916	.644	.755	.817	72
r: V≷N;	.738	.838	.866	1.008	.785	.939	.721	.888	.845	69
w: } Av.; r.t.										—141
= 0	.835	.938	.830	.716	.907	.782	.656	.887	.811	62
r: VIIIN;	.727	.977	.874	.809	.886	.717	.904	.807	.843	95–157
w: } Av.; r.t.										
									grand	total 298

in 4th, with a fairly regular ascent to 1.089 in the 8th; W. is irregular, with a minimum of .803 s in the 2nd, and a maximum 1.042 s in the 6th; S. shows a fall from 1.157 s in the 1st to 1.038 s in the 3rd, with an irregular course for the remaining intervals. For F, the course of values is irregular, varying from minima of .738 s and .785 s in the 1st and 5th to a maximum of 1.008 s in the 4th.

Of the wrong judgments, the class "V ≷ N" or "unlike" shows the following: P., a fairly regular descent from 1.286 s in the first to 1.140 in the 3rd, with an irregular ascent to 1.351 s in the 8th; G., a slight drop from 1.069 s in the first to 1.040 s in the 4th, with a slightly irregular course of higher values for the remaining intervals; W., an irregular course of values ranging from minima of .736 s and .818 s in the 1st and 7th, to a maximum of 1.307 s in the 4th.

The other class of wrong judgments, "VIIIN" or "like," yield similar results. We have in case of P., a fall from 1.338 s in the 1st to 1.237 in 5th, with a rise from 1.350 s in the 6th to 1.396 in the 8th; in case of G., a fall, a little irregular, from 1.082 s in the 1st to 1.045 s in 5th, with irregular higher values for the remaining three; in case of W., an irregular ascent from .803 s in the 2nd to 1.042 s in the 6th, with an irregular drop to 1.014 in 8th.

Taking the average for all the intervals, we see that right judgments are shorter on the whole than wrong judgments, being for the different reagents as follows: P., 1.135 s for right and 1.304 s for wrong; G., 1.231 s and 1.250 s, respectively; W., 709 s and 901 s, respectively; S., 956 s and 1.118 s, respectively; F., .817 s and 845 s, respectively; "VIIIN" or "like" judgments are longer than "unlike," "V ≷ N" judgments (P., 1.311 s and 1.298 s; G., 1.083 s and 1.074 s; W., .917 s and .868 s.)

Passing to the results tabulated for $\Delta = 0$ gm., in which norm and comparison stimuli are the same, we find about the same things as we did for $\Delta = \pm 25$ gm., though the tendencies there noted are not so pronounced in the present instance. P.'s figures for right judgments fall irregularly from 1.271 s in the first to 1.158 s in the fourth, and then rise quite regularly during the rest of the intervals to a final of 1.293 s; G.'s results are similar to these, decline a little irregularly from 1.041 s in the first to .983 s in the 5th, with a small regular ascent to 1.057 in the 8th; W's course is irregular, with minima of .648 s in the 2nd, and .578 s in the 4th and a maximum of 1.030 s in the 7th. S.'s course is likewise irregular with a minimum of .988 s in the 2nd and a maximum of 1.207 s in the 8th. F. has a minima of .716 s in the 4th, after an irregular decline from .835 s in the 1st, with another minimum of .656 s in the 7th interval.

For the average of both classes of wrong judgments, we find for P., an irregular fall from 1.287 s in the 1st to 1.168 s, in the 5th, with an irregular rise to 1.293 s in the 8th; for G., a quite regular decline from 1.057 s in the 1st to 1.036 s in the 4th, with an irregular ascent to 1.068 s in the 8th; for W., an irregular course with a minimum of 815 s in the 2nd, and another of .892 s in the 7th and a maximum 1.007 s in the 6th; for S., an irregular fall to .976 s in the 4th, with irregular higher values in the latter intervals. For F., an irregular course with a minimum of .727 s in the 1st and another of .717 s in the 6th, and a maximum of .977 s in the 2nd and one of .907 in the 7th.

Of the two classes of wrong judgments, "V⟨N" give us for P. a fall, quite regular from 1.293 s in the 1st interval to 1.128 s 5th, with an abrupt, irregular ascent to 1.303 s in the 8th; for G, a fairly regular fall from 1.070 s in the 1st to 1.029 in the 4th, with a fairly regular rise to 1.062 in the 8th; for W., an irregular course of .832 s in the 1st and 847 s in the 6th as minima with a maximum of 1.183 s in the 3rd. "V⟩N" judgments give in case of P. a fall, regular, from 1.283 s in the 1st to 1.134 s in the 6th, with somewhat higher values in the remaining two intervals; for G., a rise and fall to a minimum of 1.041 in the 4th, and irregular higher values in the remaining four; for W., an irregular course with a minimum of .539 s in the 4th.

Comparing the averages for all eight intervals of time taken together we find again right judgments shorter than wrong: (P. 1.231 s and 1.250 resp.; G. .916 and 1.079 resp.; W. .827 s and .967 s resp.; S., 1.027 s and 1.113 s, resp.; F., .811 s. and .843 s resp.)

Of the two classes of wrong judgments, "V ⟨ N" are longer than "V ⟩ N" in case of P (1.25 s and 1.249 s resp.) about the same in case of G (1.052 s and 1.051 s resp.), longer for W (1.011 s and .812 s resp.)

Now for △=O, "VIIIN" judgments are right judgments; and while as noted above these are shorter than wrong judgments, yet the peculiar nature of "like" judgments again is seen in the fact that relative to the wrong judgments the right for △=O are higher than for △=± 25 gm., the ratios being respectively as follows: P 1.01 and 1.15; G. 1.02 and 1.17; W, 1.17 and 1.27; S, 1.08 and 1.17.

The reaction times have also been calculated for the totals of the three classes of judgments which the introspective records developed, viz.: "absolute," "mixed" and "direct;" and the same are presented in the following table:

TABLE III.

Reaction Time in Relation to Nature of Judgment.

△ = ± 25 gm.

				Wrong.			
	Nature of Judgm't.		Right (V≷N)	V≶N	VIIIN	Av.	Av. for all.
Dr. P.	Absolute;	rt.	1.098s	1.219s	1.262s	1.237s	1.153s
	Mixed;	"	1.187	1.356	1.340	1.351	1.279
	Direct;	"	1.119	1.300	1.364	1.320	1.256
Mr. G.	do	"	.864	.955	.937	.945	.940
	do	"	1.001	1.238	1.227	1.236	1.121
	do	"	.896	.983	1.086	1.048	1.024
Mr. W.	do	"	.668	.886	.789	.881	.755
	do	"	.725	1.017	1.122	1.055	.870
	do	"	.863	1.217	.918	1.045	.919
Mr. S.	do	"	.945			1.068	.974
	do	"	1.065			1.200	1.124
	do	"	.907			1.045	1.000

△ = 0 gm.

				Wrong.			
	Nature of Judgm't.		Right (VIIIN)	V < N	V > N	Av.	Av. for all.
Dr. P.	Absolute;	rt.	1.212s	1.238s	1.242s	1.240s	1.229s
	Mixed;	"	1.258	1.268	1.250	1.259	1.261
	Direct;	"	1.225	1.228	1.259	1.248	1.241
Mr. G.	do	"	1.006	1.039	1.048	1.043	1.031
	do	"	1.050	1.067	1.056	1.062	1.058
	do	"	1.024	1.054	1.055	1.054	1.045
Mr. W.	do	"	.820	1.021	.920	.994	.928
	do	"	.726	1.103	.659	.912	.798
	do	"	.997	1.244	1.360	1.272	1.140
	Immediate	"	1.098			1.121	1.102
Mr. S.	do	"	1.110			1.165	1.142
	do	"	1.265			.951	1.048
	do	"	1.100			1.001	1.045

For $\triangle = \pm 25$ gm. we note that, both right and wrong judgments taken together, the reaction time of "the absolute" is the least, the distribution for the "absolute" "mixed" and "direct" is: P., 1.153 s, 1.279 s, and 1.256 s; G., .940 s, 1.121 s, 1.024 s; W, .755 s, .870 s, and .919 s; S, .974 s, 1.124 s, 1.000 s.

Right ("V ≶ N") absolute judgments are also less than right judgments of either "the mixed" or "direct" class, with one exception, S. with whom "direct" are shortest. Right judgments are also shorter than the average of both classes of wrong judgments. Thus in case of P., the reaction times in "absolute," "mixed" and "direct" respectively are for right judgments, 1.098 s, 1.187 s, and 1.119 s, and for wrong, 1.237 s, 1.351 s, 1.320 s; in case of G., .864 s, 1.001 s, and .896 s for right, and for wrong, .945 s, 1.236 s, 1.046 s; W., .668 s, .725 s and .863 s, for right, and .881 s, 1.055 s and 1.045 s for wrong; S, .945 s, 1.065 s and .907 s for right and 1.068 s, 1.200 s, 1.045 s. The "mixed" right and wrong judgments are the longest with one exception, W., for whom "direct" right judgments are the longest. This seems to indicate that a complication of memorial images with verbal supplementation interferes in a measure with the comparison of the normal stimulus, so far as reaction time can be taken to indicate anything in the matter.

Taking the average for both kinds of wrong judgments, we find in all cases the "mixed" to be the longest and with one exception (S.), the "absolute" to be the shortest, the results being as follows: P., 1.237 s, 1.351 s, and 1.320 s for "absolute," "mixed" and "direct" respectively; G., resp. .945 s, 1.236 s, 1.048 s; W., resp., .881 s, 1.055 s, 1.045 s; S., resp., 1.068 s, 1.200 s, 1.045 s.

For "V ≶ N" wrong judgments (unlike), we have in case of P., the absolute shortest (1.219 s) and the mixed longest (1.356 s); in case of G., the same ("absolute," .955 s and "mixed" 1.238 s); in case of W, the "absolute" shortest (.886 s) and "direct" longest (1.217 s).

The results show that for "VIIIN" wrong judgments (unlike) the absolute are in all cases the shortest, being for P., 1.262 s; for G., .937 s and for W., .789 s; and that in case of P., "direct" are longest (1.364 s), in case of G., "mixed" are longest (1.227 s), and in case of W., mixed are also longest (1.122 s).

The results tabulated for $\triangle = 0$ g. are on the whole like those for $\triangle = \pm 25$ gm., though less pronounced. Referring to the averages for all judgments (both right and wrong), we find the "absolute" to be the shortest with one exception W., and the "mixed" the longest with one exception W., the several values being these: P., "absolute," 1.229 s; "mixed," 1.261 s; "direct," 1.241 s; G., resp. 1.031 s, 1.058 s, 1.045 s; W. .928 s, 798 s, 1.140 s resp.; S., 1.142 s, 1.048 s, 1.045 s, resp. And the values of the "absolute" judgments are on the whole higher for △ = 0 gm. than for △ = ± 25 gm., though this relation does not hold true in case of "mixed" and "direct."

Of the right judgments ("VIIIN"), "the absolute" are with one exception (W.), the shortest, and with one exception (W.), the "mixed" are the longest. And the values of the right judgments are on the whole considerably higher there than they are for △ = ± 25 gm.

Passing to the average of wrong judgments, we note that the "absolute" are shortest for P. and G., and the "direct" for W., and the absolute for S.

The "V⟨N" wrong judgments are shortest in case of the direct for P.; in case of the "absolute," for both G. and W. The longest of these judgments appears among the "mixed" for P. and G., and among the "direct" for W.

Of the "V⟩N" wrong judgments, the "absolute" are shortest for P. and G., and the "mixed" for W.; the direct are the longest for P. and W., and the "mixed" and "direct" about the same for G.

B. *Effect of Length of Memory Interval on Mean Variation.* The mean variations have been calculated for the various intervals and for total values, not only to obtain some idea of the trustworthiness of the data presented, but also to see if any additional light was thrown on the nature of the judgment-process itself. For these purposes the following table is annexed:

TABLE IV.

Relative Mean Variation $\left(\frac{m\,v}{v}\right)$ *in relation to the Time Interval.*

t. i.	20	30	40	50	60	80	100	120	Av.
△ = ± 25 gm.									
Dr. P.									
r: V≷N	20+%	19+%	16+%	16—%	14+%	15+%	14+%	16+%	16+%
V≶N	22+	23+	18+	18—	17+	17—	15+	21—	19+
w: VIIIN	23—	24+	21—	23+	19+	17+	16+	17+	24—
Av.	23—	23+	19+	19—	17+	17—	16—	15+	19—
Av. for r.+w.	19+	21+	17—	16+	15+	16+	16+	17—	15+
Mr. G.									
r: V≷N	20—	17+	15+	16—	14+	13+	16+	14+	15+
V≶N	21+	22+	20—	18+	17—	14+	16—	16—	18—
w: VIIIN	23+	18+	20—	21—	17+	16+	14+	12+	19+
Av.	22+	20—	20+	19+	18—	15+	15—	16+	20+
Av. for r.+w.	21+	18+	17+	19—	15—	13—	16+	14—	16+

TABLE IV.—*Continued.*

t. i.	20	30	40	50	60	80	100	120	Av.
Mr. S.									
r: V≷N	17+	14—	13—	15+	18—	18+	21+	19+	16—
w: V≶N									
V⫴N									
Av.	21+	22—	17—	15—	23—	20+	22—	20+	18+
Av. for r.+w.	18—	15—	14—	17+	20+	21—	11+	19+	17+
Mr. W.									
r: V≷N	17+	12—	20+	13+	13—	18+	13+	14+	14+
w: V≶N	13+	14—	12+	13+	9+	12+	10+	9+	13+
V⫴N	16—	12—	16+	18+	12—	14+	9+	13—	17+
Av.	9+	10+	12+	14+	10+	12+	10+	16+	14—
Av. for r.+w.	20+	11+	16+	15+	11+	18+	14—	15—	14—
Mr. F.									
r:	23—	16—	20+	23+	18+	15—	17—	19+	18+
w: V≶N									
V⫴N									
Av.	18—	19—	17+	16+	21—	19—	22+	20+	17—
Av. for r.+w.	22+	17+	19—	19+	20+	17—	21—	16+	19+
△ = 0 gm.									
Dr. P.									
r: V⫴N	23—	21+	20	17+	15+	17—	16+	15+	18+
w: V<N	21+	24—	20—	19+	17—	18—	19+	20—	19+
V>N	23+	21—	22—	18+	17+	19—	17+	16—	17+
Av.	23—	19+	21+	22—	18—	17+	19—	19+	20—
Av. for r.+w.	20—	22—	20+	20—	17—	18—	16+	18+	19+
Mr. G.									
r: V⫴N	20—	22+	21—	18+	16+	17+	15+	18—	19—
w: V<N	22+	21+	24—	20—	19—	21—	23+	22—	21+
V>N	19+	20—	23+	23—	10+	17+	20—	19—	19+
Av.	21—	20+	24—	21+	19+	19—	21—	20+	20—
Av. for r.+w.	20+	22—	20+	18+	16+	20—	21+	20—	18+
Mr. W.									
r: V⫴N	11—	25—	16+	12+	12+	16—	24+	10+	18—
w: V<N	10+	15+	14+	14+	16—	11—	12—	15+	13+
V>N	6—	11—	20—	10+	20—	—	15—	12+	18+
Av.	15+	14+	17+	13+	15+	15+	15+	12+	16—
Av. for r.+w.	13+	16+	18—	18+	16+	18+	18+	13+	17—
Mr. S.									
r: V⫴N	19+	22—	17—	15—	18—	16—	20+	19+	18—
w: V<N									
V>N									
Av.	18—	16+	20+	21+	19—	18+	17+	22—	20—
Av. for r.+w.	17+	18+	19+	21+	17—	21—	18+	18+	18+
Mr. F.									
r: V⫴N	16+	20+	22—	16+	15—	14—	19+	20+	18—
w: V<N									
V>N									
Av.	22—	17+	15+	20+	18+	21+	17—	16+	20—
Av. for r.+w.	18—	16+	18+	21—	17—	16+	21+	18—	17—

△ = ± 25 gm: For the two most important subjects, P. and G., there is, taking the average for all the intervals, less variation for right than for wrong judgments, and for S. the same is true; for W., the variation is equal in the two instance; for F. it is less in wrong judgments. Between the two classes of wrong judgments, "V ≷ N," and "V III N," the variation is less in the former than in the latter for the three reagents, P., G., and W. This result is taken to confirm the conclusion previously drawn from the reaction time of the two classes of wrong judgments. As noted before, "like" judgments have an element of uncertainty in them depending upon the absence of the criteria available in the other classes of judgments, with the consequence that they are not only longer, but also more variable.

As regards the relation between the different time intervals and their respective mean variations, no evidence is forthcoming of the influence of a factor having the definite relation to the time that the memorial image has been assumed to possess. For right judgments, we have in case of P. a fairly uniform drop from 20+% to 14+% during the first five intervals, with an average above 14+% for the remaining three. This follows closely the relation between reaction time and the time brought out in table I, the minimum being 1.113s for the 4th interval (50 sec.). For G., we have a more or less constant decline from 20—% to 13+%, during the first six intervals, and a higher average for the remaining two. We have here again a close parallel to the course of the reaction time, though it should be noted that the minimum reaction time is reached two intervals earlier (in the 4th). For W., no definite tendency is apparent, the minimum of 12—% occurs in the 2nd interval and the maximum of 20+% in the third. For S., there is a decline from 17+% to 13—%, during the first three intervals, with an irregular higher level maintained during the remaining five. In case of F., the course is irregular throughout, with a minimum of 15—% in the 5th interval and a maximum of 23+% in the 4th.

Taking both classes of wrong judgments together, we find there is a fairly regular decline in case of P. from 23—% in the 1st to 15+% in the last; in case of G., a fall from 22+% to 18—% during the first five, and a somewhat lower level for the remaining three; for W. a gradual increase from 9+% to 14—% in the first four, and an irregular course for the last four; for S, an irregular decline from 21+% in the 1st to 15—% in the 4th, with a considerably higher level for the remainder; for F., the course is irregular throughout.

Of the two classes of wrong judgments, "V ≷ N" judgments ("unlike" judgments) yields the following results: For

P. there is a more or less graded decline from 22+% to 15+% during the first seven intervals, with a sudden jump to 21—% in the 8th; for G. there is a level of about 20% for the first three, a decline from 18+% to 14+% in the next three, and a level of about 16% for the last two; for W., the course is irregular, values ranging from a minimum of 9+% in the 5th, to a maximum of 14—% in the 2nd; for S. and F., wrong judgments have not been separated into the two classes, because the number of cases was thought too small. The other class of wrong judgments, "VIIIN" or "like" judgments, furnishes the following: for P., the course maintains a level of about 23% during the first four intervals, with a gradual drop from 19+% to 16+% during the next three, and a jump to 19+ in the 8th; for G., there is during the first four a fairly constant level of about 20% with a fairly regular decline from 17+% to 12+% during the last four; for W., the course is irregular as in the case of the other class of wrong judgments, though the average is somewhat higher (17+% as against 13+%). The net result of the foregoing, so far as wrong judgments are concerned, is that no influence capable of a precise mathematical formulation is observable, though for the two most important reagents, P. and G., we are entitled to say that the mean variation is higher for the earlier intervals, though in the 8th interval high values again appear in P.'s case.

Taking the averages for both right and wrong judgments, we have in case of P., a level of about 20% during the first two intervals, and slight variation around 16% during the other six; in case of G., an irregular decline from 21% in the first to 13—% in the 6th, with a drop from 16+% to 14—% in the last two; in case of W., an irregular course, with values ranging from a maximum of 20+% in the first to a minimum of 11+% in both the 2nd and the 4th; for S., an irregular course with a maximum of 20+% in the 5th interval and a minimum of 11+ in the 7th; for F., an irregular course with values higher in the latter intervals.

△ = 0 gm. In the case of right judgments, which for this increment are "VIIIN" judgments, we note the following: P.'s results follow the same general law as in the interval of △ = ±25, viz., a fairly uniform decline from 23—% in the first interval to 15+ in the 5th, and G., the same, the values for both reagents being higher than the corresponding values for △ = ±25; W.'s results are again irregular throughout, ranging from minimum of 11—% in the first and 10+% in the 8th, to a maximum of 25—% in the 2nd and 24+% in the 7th, with an average variation for the eight intervals of 18—%; S. shows an irregular decline from 19+% in the first to 15—% in the 4th, and an irregular course of higher values beyond this; F. has irregular maximum values in the first three and last two intervals, and considerably smaller minimum values for the middle intervals. On the whole the peculiar nature of "VIIIN" judgments is again manifest in the higher relative mean variations.

Taking both classes of wrong judgments (V⟨N and V⟩N) together, we find that the average mean variation for the eight intervals is higher than the corresponding mean variations for right judgments in the case of P. (20—% and 18+%), higher in case of G. (20—% 19—%), less in case of W. (16--% and 18—%), higher in case of S. (20—% and 18—%), higher in case of F. (20—% and 18—%). Comparing these results with the corresponding ones for △=± 25 gm., we get the following: P.'s results are about the same (20—% and 19—); so likewise G.'s (20—% and 19—%); W. higher for △=O (16% and—14%); S. higher for △=O (20—% and 18+); F. higher for △=O (20—% and 17—%). By comparing the two weights intervals in respect to the average variation for all judgments taken together, the following results appear: P. is more accurate for △=± 25 gm. (15+% and 19+%); G. the same as P. (16+% and 18+%); W. is equally accurate in both intervals (14—%). S. is more accurate for △=± 25 gm. (17+% 18+%); F., about the same for both (19+ and 19—). On the whole, therefore, the reagents show greater constancy for the larger weight intervals.

Of the two classes of wrong judgments, "V⟨N" and "V⟩IV," neither show very decided tendencies. With P. there is for "V⟨IV" judgments a drop from 21+% and 24%—respectively in the first two, to 17— in the 5th and then a gradual rise to 20—% in the 8th, and for "V⟩IV" judgments an irregular fall from 23+% in the 1st to 17+ in the 5th, and a drop from 19—% in the 6th to 16— in the 8th; in case of G., "V⟨N" judgments have an irregular course, giving values ranging from 19—% in the 5th to 24—% in the 3d, "V⟩N" judgments likewise irregular with a minimum of 17+% in the 6th and a maximum of 23+ in the 3d; for W., "V⟨N" judgments are irregular, with values ranging from 10+% in the 1st, to 16—% in the 6th, while "V⟩N" have a lower minimum of 6—% in the 1st and a higher maximum of 20—% in the 3rd and 5th. Taking the mean variation for the full eight intervals we find the mean variation of "V⟨N" judgments higher than the mean variation of "V⟩N" judgments in case of P. (19+% and 17+%), higher in case of G. (21+% and 19+%), less in case of W. (13+% and 18+%).

The results for the mean variations of both right and wrong judgments taken together are these: P.'s figures follow the tendency noted for △= ± 25 gm, of declining to a minimum in the fifth interval with a slight rise for the remaining three intervals; though the tendency is less obvious, we have the same general features in the course G.'s figures; while with the remaining three reagents the course is irregular throughout. The mean variation for the 8 intervals considered as a whole is higher for $\Delta = 0$ gm. than for $\Delta = \pm 25$ gm. in case of P. (19+% and 15+%), G. (18+% and 16+%), W., (17—% and 14—%), and S. (18+% and 17+%), but the opposite for F. (19+% and 17%).

The mean variations have also been calculated for the totals of the three different classes of judgments, "absolute," "mixed," and "direct," and are embodied in the following table.

TABLE V.

Relative Mean Variation $\left(\frac{mv}{v}\right)$ *for the different kinds of Judgments.*

$\Delta = \pm 25$ gm.

Nature of Judgment.	Re-agents.	Right $V \gtrless N$	Wrong.			Av. for all.
			$V \lessgtr N$	$V = N$	Av.	
Absolute,	Dr. P.	15+%	19—%	20—%	18+%	17+%
Mixed,		17—	21+	24—	22+	21—
Direct,		18—	22+	18+	21+	20+
do	Mr. G.	16—	17+	19+	18—	17—
do		17+	18+	21+	20—	18+
do		15+	20—	18+	19+	17+
do	Mr. W.	15—	12+	15—	12+	12+
do		16—	12+	—	11—	16+
do		18+	—	—	20+	19+
do	Mr. S.	15+			17—	18—
do		18+			24—	20+
do		16+			18—	17+

$\Delta = 0$ gm.

	Reag'ts	Right $V = N$	Wrong.			Av. for all.
			$V < N$	$V > N$	Av.	
Absolute,	Dr. P.	17—	19—	20+	21—	18+
Mixed,		19+	21+	22—	18+	20+
Direct,		24+	15+	18—	16+	17—
do	Mr. G.	18+	18+	21—	20—	20+
do		19+	20—	21—	18+	19+
do		21+	24—	20+	22—	22+
do	Mr. W.	15+	14—	16+	15—	16+
do		18+	12+	8+	18+	19+
do		18+	—	—	11+	15+
do	Mr. S.	17—			18+	16—
do		19+			23+	21—
do		18+			13—	16—

The figures recorded above agree quite closely in their import with those given in the preceding table. For $\Delta = \pm 25$ we find that right judgments are on the whole less variable than wrong judgments. Thus in P.'s case the variation is 15+, 17—%, 18—% for right judgments, against 18+%, 22+%, 21+% respectively for wrong judgments; G., 16—%, 17+%, 15+%, against 18—%, 20—%, 19+%, respectively; S., 15+, 18+, 16+, against 17—%, 24—%, 18—%; but in case of W. the values are anomalous, being 15—%, 16—%, 18%, against respectively, 12+%, 11—%, 20+%. Between the two classes of wrong judgments, "VIIIN" (like) judgments show higher values as a rule than "V ≷ N" (unlike) judgments, being for P., 20—% to 19—%, 24—% to 21+%, 18+% to 22+%; for G. 19+% to 17+%, 21+% to 18+%, 18+% to 20—%; for W., 15—% to 12+%. Right "absolute" judgments show as a rule least variation as compared with other right judgments: thus for "absolute" right judgments the mean variation is 15%+; for mixed, 17—%; for direct, 18—% in case of P.; 16—%, 17+%, and 15+ respectively in case of G.; 15—%, 16—%, 18+% in case of W.; 15+%, 18+%, 16+% in case of S. The exception here of 15+% for "direct" judgments in case of G., does not invalidate the general statement, for the number of direct judgments is comparatively small.

Of wrong judgments, the "unlike," "V ≷ N," follow the same general rule; for P., "absolute" have a mean variation of 19—%; "mixed," 21+%; "direct," 22+%; for G., 17+%, 18+%, 20—% respectively; but the other class of wrong judgments, "VIIIN" (like) is anomalous, the results being: P., "absolute" 20—%; "mixed," 24—%; "direct," 18%; G., "absolute," 19+%; "mixed," 21+%; "direct," 18+%, though it should be noted that in both instances, the mixed judgments have a higher mean variation than the absolute.

The interval △=0 gm. shows less decided tendencies than the interval △=±25, just discussed. Right judgments are not on the whole less variable than wrong: thus for P., we have for right judgments, 17—%, 19+%, 24+%, against 21—% 18+%, +16% for wrong; in case of G., 18+%, 19+%, 21+%, for right against 20—%, 18+%, 22—% resp., for wrong; in case of W., 15+%, 18+%, 18+% for right against 20—%, 18+%, 22—% resp., for wrong; in case of S., 17% 19+%, 18+%, for right judgments against 15—%, 18+%, 11+%, resp., for wrong judgments. Comparing the different kinds of judgments, we find consistent results among the right judgments. The mean variations are respectively 17—%, 19+%, 24+%, for the "absolute," "mixed," and "direct" in case of P.; 18+%, 19+%, 21+% resp., in case of G.; 15+%, 18+%, 18+% resp., in case of W.; and 17—%, 19+%, 18+% in case of S. As compared with the corresponding values for the difference of △= ± 25 gm., the results are somewhat higher, bringing to light again the peculiar nature of "like" judgments ("VIIIN").

Taking the totals for the different classes of judgments, we find the mean variation to be less for "absolute" judgments than it is for "mixed" with one exception, G.; the value being P. 18+% and 20+% resp.; for G., 20+% and 19+resp.; for W., 16+%, and 19+%, resp.; for S., 16—%, and 21—% resp.

C. *Effect of Memory Interval upon the Percentage of Right and Wrong Judgment.* Herewith are appended the results obtained under the above head.

TABLE VI.

Percentage of Right and Wrong Judgments for the different intervals of time.

t. i.	20	30	40	50	60	80	100	120	Percentage of all judgments.
△ ± 25 gm.									
Dr. P.									
r. V≷N	42	34—	63—	61+	52—	46+	52—	53+	51+
w. { V≶N	30	38—	19—	26—	34+	33—	22+	19—	27—
VⅲN	28	28+	18+	13+	14+	21—	26—	26—	22—
Mr. G.									
do	57—	53—	67—	69+	53+	64+	58+	49+	59+
w, { do	20+	21—	12+	15+	15—	16+	17+	26+	18—
do	23—	26+	21+	16—	32—	19—	24+	24+	23—
Mr. W.									
do	37+	60—	56—	63—	56—	57+	56—	61—	58—
do { do	37+	22+	44+	26—	32+	32+	26—	25—	30+
do	25—	18+		11+	12—	11—	18+	14+	12+
Mr. S.									
do	73+	74+	70+	61+	62+	57—	50	45+	63+
do { do									
do	27—	26—	9—	39—	38—	43+	50	55—	37—
△ = 0 gm.									
Mr. G.									
r. VⅲN	49+	38+	53—	41+	39—	27—	23+	15—	34+
w. { V<N	27+	35+	21+	26—	30+	32—	36—	48+	33—
V>N	24—	27—	26+	33+	31+	41+	41—	37+	33+
Dr. P.									
do	45	41	50	37+	32+	29+	18—	9+	32—
do { do	21	21—	24+	29—	34—	35—	41—	36+	11—
do	30	38+	25+	34+	34+	36+	41+	55+	37+
Mr. W.									
do	69	31+	49—	33+	31+	50	53+	46+	46—
do { do	25	39—	40+	40—	41—	40	28+	38+	37+
do	6	30+	11+	17—	28—	10	18+	15+	17+
Mr. S.									
do	52—	45—	48+	33+	37+	36+	35	35+	39+
do { do									
do	48+	55+	42—	67—	62+	64—	65	65—	61—

The percentage of right cases does not seem to follow any very definite tendency in relation to the time interval. P.'s result shows an irregular rise from 42% in the first interval to 63—% and 61+% in the 3rd and 4th respectively, with a fairly level of course values of about 10% less for the remaining intervals. G. shows much the same thing; beginning with 57—% in the first interval and rising 67—% and 69% in the 3rd and 4th, with a rise and fall of lesser values to a minimum of 49+ in the 8th. W. begins with 37+% in the first interval and rises irregularly to 63—% in the 4th, and then has a course of values ranging between 56—% and 61—% in the remaining four intervals. In case of S., we have after the first two intervals, a fairly regular decline to 45+% in the eighth.

Comparing the two classes of wrong judgments with each other, we find in case of P. the percentage of wrong judgments on the whole higher for the "V ≶ N" or unlike, than for "VIIIN" or like, the opposite of this in case of G. but the same in case of W.

For △ = 0 we note the following results. With right judgments, which in this instance are "like," we have in case of P., the maximum of 50% in the third interval with a fairly regular decline during the remaining intervals to a minimum of 9+ in the 8th; in case of G., a maximum of 53—% in the third, and a fairly regular fall thereafter to 15—% in the 8th; in case of W., an irregular course, beginning with a maximum of 69—% in the 1st and falling to 31+% in the 2nd and again to 31+% in the 5th; in case of S. an irregular decline from 52+% in the 1st to 35+% in the 8th. The percentage of right judgments is on the whole less for △ = 0 than for △ = ± 25 gm.

Of the two class of wrong judgments, the "V > N" show a higher percentage than the others in case of P. (37+% and 11—% respectively), about the same in case of G (33— and 33+ resp.), and a lower percentage in case of W (17 + and 37+ resp).

In the following table will be found the distribution of the percentage of right judgments for the different intervals of time among the "absolute," "mixed," and "direct" in case of P. and G.

From the above table we see that in case of P. for △ = ± 25 gm., 57—% all right judgments are "absolute," *i. e.*, those in which comparison is mediated by verbal supplementation, while only 2—% are "direct," *i. e.*, those in which there is an evident reproduction of memorial images with little or no classification of the weights. Likewise for G., 60+% all right judgments are "absolute," while only 2+% are "direct."

Comparing the different time intervals, we find a fairly regular increase in the percentage of right judgments in case of P.

TABLE VII.

Percentage of Right Judgments for the different Intervals of Time in the "Absolute," "Mixed" and "Direct" classes.

t. i.	20	30	40	50	60	80	100	120	Total.
Dr. P.									
△ = ± 25 gm.									
Absolute,	40+	46—	51—	54—	58+	61—	69—	68—	57—
No.	17	17	43	49	38	43	55	50	312
Mixed,	55—	49—	46—	44—	41+	39+	31+	32+	41—
No.	23	18	39	40	27	28	25	24	224
Direct,	5—	5+	3+	2+	0	0	0	0	2—
No.	2	2	3	2	0	0	0	0	9
△ = 0 g.									
Absolute,	38+	39+	41—	54+	61—	63—	61—	64+	51—
No.	16	18	20	25	28	30	14	7	158
Mixed,	50	48—	43—	28+	33+	35—	35—	36—	39—
No.	21	22	21	13	15	17	8	4	121
Direct,	12—	13+	16+	17+	6—	2+	4+	0	10+
No.	5	6	8	8	3	1	1	0	32
Mr. G.									
△ = ± 25 gm.									
Absolute,	49—	52	50	51—	62+	69—	71—	79+	60+
No.	22	26	32	42	29	46	51	42	290
Mixed,	44+	44	45+	47—	38+	31+	29+	21—	38—
No.	20	22	29	39	18	21	21	11	181
Direct,	7—	4	5+	2+	0	0	0	0	2+
No,	3	2	3	2	0	0	0	0	10
△ = 0									
Absolute,	47—	43—	43+	46+	48—	49—	57+	64—	48—
No.	21	18	23	18	23	19	12	14	148
Mixed,	44+	48—	43+	36—	37+	38+	43—	36+	41+
No.	20	20	23	14	18	15	9	8	127
Direct,	9—	9+	13+	18—	15—	13—	0	0	11+
No.	4	4	7	7	7	5	0	0	34

from 40+% in the first interval to 68—% in the 8th. Further, the "direct" judgment are found only in the earlier intervals. So too with G., there is an increase from 49—% in the first to 79+% in the 8th, and as with P., the direct judgments are found in the earlier intervals.

For △ = 0, the general trend is the same. P.'s results show an increase from 38+% to 64+% in "absolute" judgments in going from 1st to 8th interval, with the greater part of the "direct" judgments in the earlier intervals. G. increases irregularly from 47% in the first interval to 64—% in the 8th in the absolute judgment, and has most of the direct judgments in the earlier intervals. Passing to the totals, we find

51—% of P.'s judgments "absolute" and only 10+% of them "direct," and 48—% of G.'s "absolute" and 11—% "direct."

The percentage totals for the eight intervals of time taken together, which are given below, present some very interesting results in respect to the distribution of right and wrong judgment within each class.

TABLE VIII.

Percentage Totals of the eight Intervals of Time taken together for the different classes and kinds of Judgment.

Δ = ± 25 gm.

Re-agent.	Nature of Judgment.	Percentage of Right V⋛N	Wrong. V≶N	Wrong. V⫴N	All judgments of each reagent.
Mr. G.	Absolute,	67—	18—	17+	53—
	No.	290	67	73	430
	Mixed,	56+	19—	25+	39+
	No.	181	59	81	321
	Direct,	16+	31+	53+	8—
	No.	10	19	32	61
Dr. P.	Absolute,	60—	24—	16+	49—
	No.	312	123	86	521
	Mixed,	43+	30+	27—	48+
	No.	224	157	137	518
	Direct,	32+	46+	22—	3—
	No.	9	13	6	28
Mr. W.	Absolute,	57+	32+	11—	79+
	No.	182	102	34	318
	Mixed,	56	28	16	12+
	No.	27	15	8	50
	Direct,	56+	19—	25—	8+
	No.	18	6	8	32
	Immediate,	1—			1—
	No.	2			2
Mr. S.	Absolute,	77—	23+		60—
	No.	96	29		125
	Mixed,	45+	54—		26+
	No.	25	40		55
	Direct,	33+	67—		14+
	No.	10	20		30

Δ = 0 gm.

Re-agent.	Nature of Judgment.	Percentage of Right V⫴N	Wrong. V < N	Wrong. V > N	All judgments of each reagent.
Dr. P.	Absolute,	38—	30+	20+	43—
	No.	158	126	135	419
	Mixed,	27—	33—	40+	46+
	No.	121	148	184	453
	Direct,	30+	27—	43—	11—
	No.	32	28	45	105

TABLE VIII.—*Continued.*

Re-agent.	Nature of Judgments.	Percentage of			
		Right VIIIN	Wrong.		All judgments of each reagent.
			$V < N$	$V > N$	
Mr. G.	Absolute,	34—	35—	31+	49—
	No.	148	153	137	438
	Mixed,	36—	30+	34—	39—
	No.	127	106	116	350
	Direct,	30+	31—	39—	12+
	No.	34	35	44	113
Mr. G.	Absolute,	42—	42+	16	77—
	No.	148	150	58	356
	Mixed,	61+	22+	17—	15+
	No.	44	16	12	72
	Direct,	50	33+	17—	5+
	No.	12	8	4	24
	Immediate,	57+		43—	3+
	No.	8		6	14
Mr. S.	Absolute,	41—	59+		75+
	No.	75	110		185
	Mixed,	31—	69+		16—
	No.	12	27		39
	Direct,	45—	55+		9—
	No.	10	12		22

For the difference of ±25 gm. we find in case of P. 49—% of all his judgments are "absolute," 48+% "mixed," and only 3—% "direct;" in case of G., 53—% of all judgments are "absolute," 39+ "mixed" and but 8—% "direct;" in case of W., 79+% "absolute," 12+% "mixed," 8+% "direct;" in case of S. 60—% are absolute, 26+% "mixed," and 14+% "direct." The distribution of right and wrong judgments is this: for G., 67—% of all "absolute" judgments, 56+% of all "mixed" judgments, and 16+% of all "direct" judgments are right; for P., 60—% of all "absolute," 43+% of all "mixed," and 32+ of all "direct" judgments are right; for W., 57+% of all "absolute," 56% of all "mixed," 56+% of all "direct" judgments are right; in case of S., 77—% of all "absolute," 45+% of all "mixed," and 33+% of all direct are right.

The distribution of wrong judgments between the two classes of "like" (VIIIN) and "unlike" ($V \gtrless N$), does not follow any law for the different reagents. P. has more "unlike" than "like;" G. has more "like" for "mixed" and "direct," and about the same number of "like" and "unlike" for absolute judgments. W. has more "unlike" for "absolute" and "mixed" and more "like" for direct judgments.

For $\Delta = 0$, we have the following percentages: 1st taking all the judgments of each reagent, P., 43—% "absolute," 46% "mixed" and only 11—% direct; G., 49—%, "absolute," 39—% "mixed," and but 12+% direct; W., 77—% "absolute," 15+% "mixed," 5+, "direct," 3+% nondescript or immediate; S., 75+%, "absolute," 16—% "mixed" and 9—% "direct."

2nd. Taking the different kinds of judgments, P has 38-%, the "absolute" right, 27—% of the "mixed" right, and 30% "direct" right; G. has 34—% of the "absolute" right; 36+% the "mixed" right, and 30+% the "direct" right; W. has 42—% the "absolute" right, 61+% of the "mixed right," 50% of the "direct" right; S. has 41—% of the "absolute" right, 31—% of the "mixed" right and 45—% of the "direct" right.

3d. Of the two classes of wrong judgments, the percentage runs higher on the whole for "V ⟩ N" judgments in case of both P. and G., with the opposite relation for W. There are some miscellaneous facts related to the above which it is necessary to put into tabular form. The distinction of right judgments among the different kinds of judgments is as follows: $\Delta = \pm 25$ gm., P. has 57+ "absolute," 41—% "mixed," and 2—% direct; G. has 60+% "absolute," 38—% "mixed" and 2+% direct; W. has 71+% absolute, 12—% "mixed" and 8—% "direct;" S. has 73+% "absolute," 19+% mixed and 8—% direct. For $\Delta = 0$ gm, 51+% of right judgments are "absolute," 39—% "mixed," 10+ "direct" in case of P.; 48% absolute, 41% "mixed" and 11% "direct" in case of G.; 70—% absolute, 21+% "mixed" 5+% "direct," and 4—% "immediate" in case of W.; 77+% "absolute," 12+% "mixed," 10+ "direct" in case of S.

SUMMARY OF RESULTS.

1. The larger portion of the judgments are absolute, *i.e.*, those in which verbal supplementation is the most conspicuous part of the judgment process. This agrees with the conclusions reached by Prof. Angell in his experiments on the discrimination of shades of gray.[1]

2. There is a correlation between correctness of judgments, length of time in making the judgment, and the relative mean variation. There is a decrease in the length of the reaction, in the extent of the mean variation and an increase in the percentage of right judgments during the earlier intervals, clearly indicated in the case of the reagents, Dr. Pillsbury and Mr. Galloway, who have furnished over three-fourths of the judgments recorded. The interval of 40 to 60 secs. seems the most favorable for the judgments, so far as this is indicated by maximum percentage of right cases, minimum length of reaction time and mean variation. These results thus indicate some fundamental change in condition for judgments made with a longer interval between presentation of norm and comparison stimulus. Prof. Angell also found in the experiments above noted, that

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[1] Philos. Stud., Vol. 19, p. 21.

the reaction was shortest for 60 secs., the longest interval investigated; and explains the fact as due to a change in sensory conditions. He writes in the article already referred to: "as regards the reactions for 5 secs. it has been observed that the demeanor of the reagents for the shorter intervals, is commonly, though not invariably, different from what it is in the longer In the 30 and 60 sec. intervals, the reagent is apt, soon after the exposure of the norm, to relax the trunk muscles, settle himself in an easy attitude, to breath easily and to move the eyes from time to time over the background. During the shorter periods, as has already been observed, the reagents usually try to maintain uniform sensory conditions for both norm and comparison; the tension sensation from trunk, respiration and eye muscles are kept constant in order apparently to make the conditions of comparison as much alike as possible. Accordingly we have, for most reagents, a much larger mass of background of sensation entering into the comparison of shorter intervals, and in all probability more genuine acts of comparison."[1] I have made no records touching on the demeanor of the reagents for the various intervals; but there is a fact reported in the preceding part of this paper, which confirms the statements made in the above quotation, viz., that the percentage of absolute judgments increases in the longer intervals, while the direct judgments are relatively most numerous in the shorter intervals.

3. "Like" judgments are of a negative character, that is, such as the reagents make when the criteria of the other judgments are absent. There is an uncertainty in making judgments of this sort, which shows itself especially in a lengthened reaction time and often in a higher mean variation.

4. Right judgments are, as a rule, shorter, less variable than wrong judgments, the more so when there is a difference between norm and stimulus of comparison.

5. Absolute judgments are shorter and less variable than other kinds, and the percentage of them increases in the longer intervals. Frequently the memorial image acts as disturbing factor, as is seen in the fact that in many instances the "mixed" judgments are longer than either the absolute or direct.

6. The images are, according to the introspective reports of the reagents, more visual, or at least as much so, as they are motile. The sensations aroused by lifting weights are a complex of visual and kinæsthetic elements, of which the visual function the more conspicuously in the judgment process. When this is direct, that is a comparison between the sensations peripherally aroused by the stimulus of comparison, and the memorial image of the norm.

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[1] Philos. Stud., Vol. XIX, p. 18.

7. There is an evident loss of sensory memory with the flow of time, if by that we mean a higher percentage of errors for the longer intervals of time (beyond 30 to 60 seconds) and a lengthened time of reaction.

8. The general trend of the results is that in judging the comparative magnitude of lifted weights, the most important thing concerned in the process is not a memorial image undergoing a progressive decay that bears a definite mathematical relation to the time as measured by the amount of work necessary to restore it to its former integrity.

BRIEF STATEMENT OF THEORY OF THE JUDGMENT OF LIFTED WEIGHTS.

All reagents reported the immediate disappearance from consciousness of the memorial image, when present, as soon as the lifting of the second weight began; so that a direct comparison between the memorial image and the sensations aroused by the second weight in a way at all analogous to the comparison of two peripherally aroused sensations occupying consciousness simultaneously or nearly so, was out of the question. Besides, in these cases where the reagents were unable to pass any judgment whatever, because the norm had been completely forgotten, it was found that the failure was invariably due to an inability to reproduce the verbal supplement of "light" "heavy," etc., which served to place the norm in a scale of weight values. The classification of the norm enables the reagent to adjust the muscles for the lifting of the second weights or stimulus of comparison to a degree determined by the intensity of the strain sensation resulting from the adjustment. If this adjustment is just sufficient to lift the second weight with the usual degree of ease and rapidity, the norm and stimulus are felt to be equal; if the adjustment more than suffices, the second weight flies up more quickly than usual, and seems lighter; on the other hand, if the adjustment is insufficient, the weight does not move, a fresh innervation is made, the feeling of effort is correspondingly great, and the stimulus of comparison seems heavier. The judgment results from changes in a complex of peripherally aroused sensations, and not from a comparison between a peripherally aroused sensation and a copy of the same in the form of a memorial image. This theory is substantially that of Müller and Schuman.[1] with the exception of the part played by the "motor" image, The impulse to adjustment is largely a verbal idea, which gives a position to the weight in a scale of values.

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[1] Müller and Schuman, Pflüger's Archiv, für Physiologie, 1889, Vol. 45, pp. 55 ff. Martin and Müller, Zur Analyse der Unterschiedsempfindlichkeit, Experimentale Beiträge, J. A. Barth, Leipzig, 1899, pp. 206 ff.

ORGANIC CHANGES AND FEELING.[1]

By John F. Shepard, Ph. D.

The experimental work here reported was begun in the early part of November, 1903, and was continued until January, 1906, with omission of the summer vacations. The intention at the start was to test the organic reactions accompanying various mental processes, with special reference to Wundt's tridimensional theory of feelings, and this object has been kept in mind throughout. To this end the subjects were asked to give a careful account of the conscious factors involved, and to note particularly to what extent they might be called excitement or repose, strain or relaxation, agreeableness or disagreeableness. There certainly are such processes, whatever their character as feeling or sensation may be, whatever may be the physiological changes accompanying them, and these two points must be kept partly separate for the present. Some attempt will be made to determine more definitely with what kinds of processes we are dealing, but the success or failure of this attempt cannot destroy the value of the work on the organic reactions.

The possible expressive processes studied were the change in the volume of the hand; in the volume of the brain; in the heart rate; and to some extent depth and rate of breathing and the form and size of the plethysmographic pulse in brain and hand. The disagreement among the writers on the subject is so great that no estimate can be made of the present status of the problem except by a brief abstract of the recent literature, and to this I now turn.

It is needless to review the writings treated by Angell and Thompson in their article on "Organic Processes and Consciousness." It would be difficult to do it again so well. I can begin with a statement of the results obtained by these authors. They may be summarized as follows: when the attention process moves smoothly, respiration and circulation progress with regularity. Strained attention is accompanied by more vigorous bodily reactions than low-level relaxed attention,—but both agree in relative regularity. Breaks and shocks are followed by relatively violent bodily changes. Emotions are states of instability; attention is spasmodic and interrupted. This instability is greater in disagreeable than in agreeable states. Solving a problem that requires merely relatively strained attention is regular attention. If one becomes confused or anxious, we get irregular attention.

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[1] From the Psychological Laboratory of the University of Michigan.

No factor, vasomotor, rate or amplitude of pulse, position or emphasis of dicrotic, or rate or amplitude of breathing changes regularly in one way for agreeable and the opposite way for disagreeable conditions. Almost all emotional states, agreeable or disagreeable, produced constriction. Amplitude of pulse decreases in almost all emotional states. The rate of the heart is sometimes increased on an average, sometimes decreased, sometimes not changed at all. Increase of rate is most frequent, but the more significant fact is the irregular rate with emotions.

The results with sensory stimuli vary. The most pleasant experience (harmony) always caused constriction. Unpleasant odors caused dilation in a few instances. The rate of the heart with sensory stimuli increases about as often as it decreases.

Mental application gave a slight amount of vasomotor change, frequently so slight fluctuations as to be hardly noticeable. Respiratory rhythm disappears. Intellectual states are distinguished from the affective states by the greater regularity, and not by any uniformity in the direction of change. In about half of the mental application tests there are both vaso-dilation and vaso-constriction in a single test. When the change is in only one direction, it is an even chance that it be dilation or constriction.

Lehmann, in the first part of his work, "Körperliche Äusserungen psychischer Zustände" gives considerable space to a study of the normal condition. Perhaps the most surprising statement is that the long undulations of the volume curve are called forth by vague conscious conditions which are not able to catch the attention. He says that these waves do not appear in deep sleep, and the changes in heart-rate are very small in the gradual undulations. Such a conception of these waves is a source of error throughout the book.

A concentration of the attention, according to Lehmann, is immediately accompanied by a few quickened pulses, during which the volume has a tendency to rise; then follow four to eight slow pulses and the volume falls; finally the volume rises again with quickened pulse. Taking all these phases together, the length of pulse is always shortened. If the activity is more lasting, the volume remains nearly at the normal after the last rise, but with much shortened pulse.

With involuntary attraction of the attention by a sudden external stimulus, the arm volume usually rises slightly at first, then sinks, and lastly rises to normal. While the first pulses after the stimulus are shortened, the condition as a whole is characterized by a lengthened pulse, which shows more in the sinking than in the rising volume.

During strain of expectation, the volume of the arm is lessened, the height of pulse is small, the length of pulse varies but little from the normal. The reactions to various stimuli are obscured. If the strain is strong, external stimuli call forth only changes in the frequency of the pulse; if less, they cause a rise of volume which is soon lost. If the strain ceases, the volume rises with higher pulse.

In an indifferent mood, the bodily expressions of weak stimuli are independent of the feeling tone; if the attention to them is purely involuntary without active strain, the only change is lengthening of the pulse. If the stimulus causes some active attention, the volume changes of these states come in, but with lengthening of the pulse unless the active attention is too prominent. During a state of strain each new direction of attention is expressed by a volume change which is to be considered as a resultant of the changes which the new activity would cause of itself, and the rise of volume which is a consequence of the lessening of the strain. It is difficult to accept the author's explanation that, with maximal strain, the two forces arise side by side so that the volume remains nearly unchanged, while with weaker strain, one gets a predominating rise of volume.

Disagreeable sensations cause a strong and persistent fall of volume with decrease in the height of the pulse as well as its length. If the volume rises again, the height of the pulse rises also. With weak disagreeableness, the pulse begins to lengthen as the volume rises; with strong disagreeableness it shortens still more during the first of the rise and at the original level of volume the pulse length is usually markedly less than the norm.

A depressed mood is distinguished by the marked undulations. Agreeably toned sensations and other pleasant conditions, not very complex, express themselves by increase in the height and lengthening of the pulse; while the volume usually only immediately at the beginning shows a slight fall and then quickly rises above the original level. But one generally does not find all these changes in the same curve.

An external stimulus must affect consciousness in order to cause organic reactions. To the degree to which another state distracts attention from it, so far its special bodily expressions vanish. An agreeably or disagreeably toned sensation suggested in a hypnotic state calls out the same bodily changes which accompany the sensation when caused by a stimulus under normal conditions, even though there may be acting a stimulus which of itself would cause different reactions if it came to consciousness.

In the second part of his book, Lehmann develops a theory of agreeableness-disagreeableness which he states briefly about as follows: when a psycho-physiological process demands no greater use of energy of a neuron than the assimilative changes are able to supply continuously, then the psychical effect will be an agreeable feeling while the physiological effect will be a stimulation of changes in other centres. The maximum of agreeableness is attained when the assimilative processes can just supply the demand. When these limits are exceeded, the agreeableness and the stimulation are both decreased; the demand in the active centres now causes an inflow of energy from the surrounding parts by which simultaneous processes in the latter are inhibited. We get then a neutral condition; and finally, if the need in the active neurons is so great that it cannot be supplied by the assimilation in connection with the intercellular stream of energy, the psychical effect will be a disagreeable feeling. An inhibition of other simultaneous processes will therefore accompany disagreeableness, except when this is of too short duration to allow an inflow to take place (as in fright).

Taking the curves published in Lehmann's atlas and the descriptions given by Lehmann of the conditions under which they were taken, Wundt attempts to show that in so far as the experience is exciting in character there is some increase in volume, increase in height of pulse, but no change in the pulse length; in so far as it is quieting, the reaction is the reverse. With agreeable states the volume is increased, the pulse heightened and lengthened; with disagreeable the reverse conditions hold. Strain gives a decreased volume, a weakened and lengthened pulse; relaxation the reverse. Wundt believes that in this way a better interpretation is obtained for Lehmann's results.

Brahn begins his article with a careful account of introspections in which he finds that Wundt's tridimensional division of feelings is justified. In his experimental work, he considers only the length and form of the pulse. He measures the single pulse beats, but very roughly, and uses only a few before and after the stimulus. He states that subliminal stimuli cause a slight lengthening of the pulse; that there are three pairs of changes in the pulse corresponding to three directions of feeling: agreeable states give longer and higher pulse, disagreeable shorter and lower; exciting higher and no change of length, depressing lower and of the same length; strain shorter and relaxation longer pulse. The changes in each case come in a few pulses after the stimulus and last several beats. There is a periodic strengthening and weakening of the expression with strain corresponding to the variations of attention.

W. Gent finds with *Spannung*—a sort of indefinite expectation—if it is of short duration, a short fall of volume, more or less lessened height of pulse, and, most prominently, lengthening of the pulse; if of longer duration, the volume curve also remains low with very small waves.

With relaxation, the volume rises above the norm, generally with respiratory waves; the pulse is shortened. No feelings can be isolated except *Spannung* and *Lösung;* in all other places we have resultants.

Concentration of the attention as found in the solution of a problem or in counting marks gave a shallow and more regular breathing, a fall of volume and decreased height of pulse, with waves; there is no constant change of pulse length. From this he thinks the feeling is a mixture of "*Spannung*" and "*Erregung*." One must question, however, whether "*Spannung*," as he uses the term, is not the complex.

So far as a state was disagreeable it gave a fall of volume and height of pulse and shortened pulse length. These changes will be more or less complicated by the strain and exciting effects present, less so if the disagreeableness is strong. Agreeableness causes a rise of volume and height of pulse with greater respiratory waves; the pulse may be lengthened or shortened during the stimulus, but there is always a shortening after the stimulus.

Gent thought to get excitement by suggesting the arm as active or that its volume should rise; depression, by suggestion of the reverse. The reaction for excitement was the rise of volume, heightened and shortened pulse. The shortened pulse was obtained only by the method of suggestion. Depression gave the reverse reaction.

P. Zoneff and E. Meumann report quite fully the results of experiments in which they are mainly concerned with the accompaniments of mental processes in the heart rate and the breathing. They study not only the rate but also the depth of both chest and abdominal breathing. Working with attention to optical, auditory and tactual stimuli and to the solution of arithmetical problems, they find that, in general, a voluntary concentration of attention causes a slowing of the pulse and an inhibition of the breathing, greater in the chest than in the abdominal breathing. The breath is much more affected by the sensory than by the intellectual attention. The waves of the attention correspond to waves in the pulse and breathing changes; with weakening of the attention the pulse frequency and total amount of breathing increase.

Turning to the agreeable and disagreeable feelings, the authors state that all agreeable processes cause a shallower and more rapid breathing, with slowing of the pulse, all disagreeable processes give deepening and slowing of breathing and a quickening of the pulse. The chest breathing is more affected than the abdominal. If we consider both the rate and depth of thoracic and abdominal to get the total amount of breathing, we see that agreeable states lessen the breathing activity, disagreeable increase it.

The effects of the feelings vanish with a distraction of the attention by another stimulus. The authors also study the results of concentration of the attention upon the stimulus and upon the feeling, and show that a mere direction of the attention to the feeling strengthens it, but it is weakened if made the object of a psychological analysis.

One of the most interesting of the articles that have recently appeared is that by M. Kelchner, on the relation of breathing and pulse changes to stimuli and feeling. After a thorough criticism of Lehmann's work, the writer passes to the experiments on agreeable and disagreeable states caused by taste, visual and auditory stimuli. Agreeable tastes give quickened pulse, agreeable tones and colors a slowed pulse. The quickening of the pulse with taste stimuli is less, the more agreeable the experience, so that longer pulse seems to be here the natural expression of agreeableness. The pulse increases in rate generally with disagreeable states. The breath changes, with both agreeable and disagreeable, show great individual differences. Pulse and breathing are to a certain extent independent variables, and there is a great difference in the significance of the two as expressions of the feelings. The individual differences are found in depth and rate changes as well as in the relative part of the chest and abdominal factors, so that it is always necessary to consider both breath curves.

Strain was studied mainly by announcing a reaction of some sort which should take place at a second signal. Quickening of the pulse was the result; the breathing showed great individual differences. Relaxation gave the reverse of the expression of strain, and this was true, even in the individual differences. If strain precedes and turns to relaxation at an agreeable or a disagreeable process, the relaxation simply displaces the other feeling reaction, and the result is indifferent to the nature or degree of the agreeableness or disagreeableness.

Cutaneous pain gave rather uneven results. There was a tendency to faster breathing. The pulse was quickened, except with one subject.

A faster pulse and an irregular, generally somewhat quickened breathing, accompanied fright. There was a slowing of the pulse afterwards, which seems to correspond to the feeling of relaxation which may follow fear.

A most careful recent investigation is that of H. C. Stevens in his article on the plethysmographic study of attention. He determines the volume changes and measures the heart rate from the plethysmographic curve,—a practice which will generally do very well, except that the method of counting the number of pulse beats in ten seconds, almost invariably used, is all too rough and meaningless.

Visual, auditory, and tactual attention, and attention to multiplication were studied. For visual attention, the rate of respiration was much increased, the pulse rate did not change consistently. With auditory attention the pulse rate was decreased, the respiratory change uneven. Both pulse and respiration were slowed for tactual stimuli. The results with multiplication were like those with the visual group, and the work was probably thought of visually. The most marked result in all attention was inhibited breathing and decreased volume of the arm.

Such variations of the results with the sense organ concerned leads the writer to the conclusion that the changes in rate of pulse and respiration are due simply to the process of sensation, not to the attention at all. And to be connected with this is the statement that "every sensory stimulus tends to produce a fall in the volume of the arm."

R. Müller has published an article in which he criticises the use of the plethysmograph for psychological purposes. The paper is well worth reading; but I do not think it necessary to review it nor to offer any reply, as I have recognized in an even more thoroughgoing way the facts that he urges.

In a recent book, Elemente der Psychodynamik, Lehmann gives a further discussion of the problem, and reports some experimental researches. After summarizing previous results and criticising the tridimensional theory, he develops his theory of feeling in connection with Verworn's theory of Biotonus. If assimilation $\div$ dissimilation $= 1.$ the psychic state is agreeable, and the stronger; the greater both assimilation and dissimilation are. If assimilation $\div$ dissimilation $< 1.$ the state is disagreeable. The Biotonus of the neurons will vary with the kind as well as the strength of the stimuli, and the value of dissimilation, with which assimilation $\div$ dissimilation becomes less than 1. will vary with different conditions of the organism. If assimilation $\div$ dissimilation $< 1.$, the effect will be to inhibit other processes, and so the vagus centre. This will give an increased heart rate, which will send more blood to the brain, and so increase the assimilation. When assimilation $\div$ dissimilation $= 1.$, the vagus centre will be stimulated and so the heart rate decreased, a result which is also teleological since the heart is relieved when great flow to the brain is not needed. "The feeling-tone is the psychical indicator of the Biotonus of the acting centre, whose bodily effects we find in the circulatory changes."

In the experimental work, Lehmann measures also the transmission time of the pulse waves, a method which is suggestive but which needs a more thorough treatment. He finds that no respiratory waves appear in the normal arm volume unless the breathing is very deep. They usually appear if the volume and height of pulse increase above normal; if they occur with quiet breathing and normal height of pulse, the subject is either sleepy or depressed. Of especial importance is the fact that he retracts his previous statement that the gradual undulations in the volume curve are due to vague conscious states. He now grants that there may be such waves without a correlate in consciousness, and which are, he thinks, due to a rhythm in the heart rate. He denies that these waves are effective in the time of transmission of the pulse wave, which means that they are compensated by vasomotor changes. But there are yet longer variations in the transmission time which are in the same direction in the carotid and radial arteries and in the opposite direction in the tibial, so that the two compensate each other and leave no trace in the volume. The results are to me, however, far from convincing.

"Every intense mental effort of considerable duration causes a greater frequency of the heart, expansion in the carotid, which probably continues after the work has ceased, and also constriction in the radial artery." This agrees with Berger's statement that concentration of the attention is connected with increased brain volume and height of pulse, which lasts until after the end of the work. Yet Lehmann finds that *Spannung* and expectation are expressed by decreased arm volume and small pulse which depend upon a constriction in the radial artery; and, in the carotid, no circulation changes can be indicated with certainty, which he connects with Berger's assertion that in *Spannung* there is no change in the brain curve, neither in volume nor height of pulse.

His results with sleep and related phenomena, seem to me very uncertain; I leave them for future consideration.

This writer finds that the characteristic of simple agreeable feelings is the expansion in the radial and the carotid; which agrees with Berger's result that agreeable processes go with a decrease in brain volume and increase in the height of the brain pulse.

To his former results with disagreeable states he adds that they are accompanied by a constriction in the brain. This he infers from the decreased time of transmission of the pulse in the carotid, which continues after the heart becomes slower. Again he agrees with Berger's statement that "disagreeably toned sensations cause an increase in brain volume and decrease in height of pulsations in the brain." This contraction of brain vessels and decreased height of pulsations, it is explained, protects against a too rapid dissimilation.

THE PERIPHERAL VOLUME CHANGES.

The first experiments were designed to test the changes in peripheral volume and the depth of breathing. It was, at first, thought that the heart rate might be measured from the same curves, but this idea was soon abandoned for reasons to be discussed later. Three forms of the plethysmograph were used at one time or another during the experiments. We may consider them separately.

Zimmermann's modification of Lehmann's plethysmograph was used during the latter part of the first year's work. While this modification is probably inferior to Lehmann's original form, there are many objections to the use of either for this purpose. It is too much of a stimulus to the subject, involves too much preparation on his part and may easily become positively uncomfortable. It is little wonder that Lehmann obtained so many abnormal results, because the reactions were preceded by a state of *Spannung*. It is with great difficulty that movements are eliminated. Every swing of the arm in breathing, every jump, every contraction of the muscles of the arm is almost certain to be directly recorded in part at least. Again, it is too directly affected by the pulse in the large blood vessels, the outflow from the large veins, and, relatively, at least, cannot record delicately changes of volume so far as these are due to constriction or dilation of the small arteries. The results obtained by its use were not in conflict with those from the other instruments, but were not nearly so clear or certain. It was therefore abandoned in favor of the other forms.

It will be convenient here to notice a change made in recording the curve from Lehmann's plethysmograph. The Marey tambour is probably universally used for this purpose. But it is also very unsatisfactory. The unequal yielding of the rubber at the sides of the disk in response to the motion of the lever, the variable tension of the rubber at different heights of the curve tend inevitably to obscure the record. For this reason a piston recorder of large size with hard rubber plunger was made and used in place of the tambour. A much more delicate response, both as recording the long waves and any reaction to stimuli was obtained by this means.

The next plethysmograph to be considered is the Hallion-

Comte, an air plethysmograph. Objection may be made to this instrument in that it does not include all of any part of the body, so that some of the record desired may be lost; and that, if it is used on the hand, the subject may involuntarily squeeze the bulb, particularly at the time of a shock, and thus obscure the result. These abrupt movements, however, may in general be easily distinguished from other changes. The apparatus is useful when the pulse in the finger alone is not sufficient to give a fairly large record, as is often the case with women, and was retained for this purpose. It was also used for the peripheral pulse later when the brain and hand (or foot) curves were taken side by side. For it is the most convenient form available in taking a long record, as was done particularly in work on sleep. It was always used with a moderate sized piston-recorder with plaster of Paris plunger. When so combined, it is the most delicate apparatus we know for studying the vasomotor waves.

We come lastly to the finger plethysmograph, described by Lombard and Pillsbury in the *American Journal of Physiology*, Vol. III. This is altogether the most satisfactory when it can be used. It is easily and comfortably operated. It reports changes which are probably as nearly vasomotor as one can obtain. Movement of the arm affects it comparatively little. It was always used with the most delicate piston recorders, indeed the tambour will write practically no tracing with it. When so adjusted, the apparatus requires more care from the operator, particularly in attending to the recorder, but requires less from the subject, and is excellently adapted to the task in hand. Instead of warm water in the outer cylinder, a towel was generally spread over hand and plethysmograph if necessary. As a result, the air in the inner cylinder was sometimes growing slightly warmer during the first tests and causing a slow rise of the needle. But in almost no case was this large enough to interfere with an accurate reading of the result if a reasonably long curve was taken.

It must be confessed that there is one objection to this arrangement: the tracing is often very faint. The pressure of the needle of the recorder upon the drum must be so light that the mark is often not heavy enough. It can be seen easily if the records themselves are studied, but cannot be published except at considerable expense. For this reason, I shall select comparatively few curves for reproduction, and in fact mainly those illustrating agreeable and exciting conditions, the most disputed reactions. This, however, can be no great deficiency, since the whole story is practically told by these few, and since I have studied with special care any curves that might possibly be interpreted as the reverse of the correlation found.

Of the breathing record there is little to be said. A Sumner pneumograph was used on the chest and connected to a Marey tambour by rubber tubing. The intention was to show the changes in amplitude of chest breathing alone. This is, to be sure, inadequate to test a possible finer correlation between breath changes and mental process, since we should, for this purpose, know the total amount of breathing in both chest and abdomen; but it is sufficient to detect any changes that might materially influence the circulation. The variations in rate, like those of heart rate, were measured by more accurate methods in later experiments.

The kymograph used in the earlier experiments was one made by the Chicago Laboratory and Supply Co. That used mostly after the first six months was the Zimmermann pattern with Hering's slide. Along with the piston recorder writing the pulse and the Marey tambour tracing the breathing wave, and just above the latter, a magnetic indicator was used to mark the time at which stimuli were given. Above this was a time marker recording seconds in the first experiments, but this was of no importance after it was decided that the heart and breath rate must be studied in another way.

Among the stimuli used at one time or another during the experiments were agreeable and disagreeable smells, agreeable and disagreeable tastes, colored lights, deep and shrill whistles, chords or discords on tuning forks, music on stringed instruments (the violin and the zither), noises, attention to counting marks or a minimal sound, to a touch or to a multiplication, recalling of emotional experiences, listening to amusing reading, etc.

The arrangement in the first experiments was as follows: The finger plethysmograph was suspended at a convenient height from the floor by the side of a table. On this table were the kymograph, the rotating standard holding the recording apparatus, the syringe and its attachments used to adjust the level of the recorder needle, the electric key connected with the indicator, and part of the phials containing smell stimuli. On a stand near the operator, as he stood facing the kymograph, were the tuning forks and other objects needed to give the stimuli. The subject sat in a comfortable position next the plethysmograph; his arm rested in the swing and his eyes were closed except when he was told to open them to count marks, etc. The second finger was generally used in the plethysmograph. The room was quiet.

After the apparatus was adjusted so that all parts were working satisfactorily, a normal record was run for some time, a stimulus was then given, after which time was allowed for recovery, the kymograph was stopped, the stopcock turned and the subject asked to give his introspection. This was written down and numbered to correspond with a number on the record. The indicator circuit was closed at approximately the time at which the stimulus was applied, although it was sometimes impossible for the operator to do this exactly.

In the later experiments the subject and operator were in different rooms. Both were dark rooms, that in which the subject sat was closed securely so that no light was visible except when an electric lamp was burning. The subject could hear practically nothing of the movements of the operator. The plethysmograph was suspended near the wall and beside a table. There was a hole leading through the wall to the other room. Through this hole passed the tubes from the plethysmograph and pneumograph to the recording apparatus which stood on a table next the wall. Through this also passed electric wires for signal keys, tubes attached to the whistles so that the operator could blow them at any time, and wires for two electric lights. What space was left through the hole was lightly packed with cloth. The electric lights were turned on or off from the operator's room. They were hung near the wall in front of the subject. Colored bulbs were made and used in them. The colors were red, yellow, green, blue, and violet, as well as the usual white. So the subject could be exposed to any of these lights or left in total darkness. A signal could also be easily given for any kind of mental work. The taking of the record and the introspections were analogous to the procedure in the first experiments.

The subjects for these experiments were Professor Pillsbury, Mr. Wright, a graduate student; Mr. Bayley, Miss Udell, and Miss Killen—senior students; and the writer. Professor Pillsbury, Mr. Wright, and Mr. Bayley served for most of the work. In about two hundred of the earliest experiments made, the introspections are insufficient to classify the experience accurately in the three directions of feeling. It was more or less a training period. The results obtained may be reviewed very briefly. And, in the first place, it may be noted that here, as in the later experiments, many tests gave no determinable result. Nor is this due to any fault of the apparatus, for undulations, and other changes when they occurred, were recorded more delicately than is usually the case. There was almost always some undulation, and the reaction was simply too weak to be distinguished from it. In truth this same fact was found with the Lehmann plethysmograph. Nor were the subjects in a state of strain; the pulse was normal, showing nothing of the constricted character it does during strain. And it might be an agreeable or a disagreeable experience which gave no response. A stimulus does not necessarily give a definite reaction. All tests that could reasonably be interpreted as giving either rise or fall of volume were selected, the others neglected as determining nothing. Those also were omitted which were introspectively unsuccessful or meaningless; and of these there was a considerable number when the more accurate introspections were required.

As illustration of a test which gives no definite rise or fall of volume, we may give Chart W 31. (Pl. 1.) The stimulus was a chord, discord, and chord in succession. There was a marked change in the breath; the whole pulse curve is slowly rising with the warming of the air in the tubes, but shows no determinable reaction to the stimuli. The vasomotor wave here is not so large as is often found. Like phenomena may be obtained with simple or complex stimuli.

Summarizing roughly the results of these preliminary experiments, we find that nineteen agreeable stimuli gave a fall of volume distinctly, while four gave a possible rise. Fifteen disagreeable stimuli gave a distinct fall, and two a possible rise. Four shocks and five attentions to a touch sensation were accompanied by a fall. There was a fall with several cases of strain of expectation. One agreeable smell falls apparently upon a Traube-Hering wave, and in the same record is a disagreeable smell accompanying at least as prominent a wave. The same is true of two chords and a discord. Only one rise seems to be due to the stimulus, and this is with an agreeable smell which has led to deep breathing, the rise is apparently only a short effect of the breathing, and is followed by a fall.

Turning now to the records with which more careful introspections were taken, I shall describe a few typical cases.

W 166. Multiplying. The work was done easily, with some effort, but no confusion. Subject pictured the number and the result. He discovered a little mistake just at the end and that caused a momentary unpleasantness, but only a little and this near the end. Strain was practically the only feeling present. The volume curve as a whole was slowly rising, the effect of the stimulus was a fall with smaller pulse. Due to the displacement of the recorder needle in its arc, the whole pulse curve should be moved back somewhat; measurement shows the fall began about two pulse beats after the stimulus was given. The breathing was slightly deepened. The subject raised his finger a little to indicate where the work was done. (Pl. 1.)

W 167. The stimulus was again multiplying, and the conditions practically the same as in W 166, except that the work required a little more effort. The breath was not much changed, somewhat shallower. The volume decreased. (Pl. 1.)

B 20. The subject was asked to attend to a faint watch tick, to get, if possible, the attention wave. The condition was one of strain only. The pulse had just about recovered from a previous fall, the result of the work was fall of volume with lower pulse. Undulations were present in the pulse during the stimulus period, breath waves before the stimulus and in the latter part of it. The volume rose with the feeling of relaxation afterwards. The breathing was distinctly shallower during the stimulus. (Pl. 1.)

W 104. The stimulus was carbon-di-sulphide. Disagreeable, no particular strain of attention, nor excitement, nor depression. The result was a fall of volume, smaller pulse, and inhibited breathing. (Pl. 1.)

S 83. Oil of violet. An agreeable sensation with no particular strain of attention and no excitement. Subject in perhaps a little strain before. The mark was pressed a little early. The result is a fall of volume with smaller pulse. (Pl. 2.)

W 89. Camphor followed by cinnamon. Both agreeable, perhaps a little excitement, did not want either to cease. Again the result is a fall of volume. The breathing was a little deeper at first. (Pl. 2.)

B 9. (The curve is not reproduced.) Cinnamon. It was announced to the subject at (a) that cinnamon would be given. The smell was given at (b). The breathing was somewhat deeper after the announcement until the smell was removed. The pulse curve was slowly rising on account of the increasing temperature in the tubes. The stimulus caused a fall of volume and somewhat smaller pulse.

P 129. The stimulus was a violet light. It was agreeable and a sort of weird, little exciting light. There was marked depression when it was turned off. So the succession was an agreeable excitement, followed by a depression, the latter perhaps slightly disagreeable. The marker current was broken a little too quickly as the operator had to move a little to turn off the light after breaking the indicator circuit. There was a fall with smaller pulse beat both at the appearance and disappearance of the light. The breathing was scarcely changed. (Pl. 4.)

P 145. This record was taken with the Hallion-Comte plethysmograph. The stimulus was a violet light. It was agreeable and arousing, although the arousing effect was not so great as with the white light. The breathing was a little deepened. There was a preliminary rise followed by a fall. This was one of the very few rises obtained. And it may well be only a somewhat larger Traube-Hering wave, a kind of appearance which quite often occurs in an indifferent condidition *as well as in deep sleep*. (Pl. 2.)

B 27. The stimulus was the major triad C, E, G. There was no surprise, the stimulus having been announced just before. It was distinctly pleasant, possibly a little arousing. No especial strain of attention. The breathing was not changed uniformly. The volume curve had been artificially dropped a little before the stimulus, to bring the needle more nearly to a level position. The stimulus caused fall of volume with lower pulse. There was some breathing and Traube-Hering wave all through. (Pl. 3.)

W 87. (The record is not reproduced.) The stimulus was again the major triad, turned to a discord in the middle, then major triad again at the last. The discord was practically indifferent, the triad agreeable. Little surprise. The volume fell during the first sounding of the triad, while the feeling was little more than agreeable.

B 6 S. This is a curve taken while the subject was listening to music by Sousa's band. The feeling was agreeable and stimulating. This gives a decided fall in the volume, with smaller pulse. The Traube-Hering wave shows all through. (Pl. 3.)

W 76. The stimulus was the major triad. It gave a decided start, the tones were pleasant. With the tendency of the tones to be felt as pleasant and of the surprise to make the whole unpleasant, it is impossible to say that this was more than a state of excitement. Indeed this seems to be about the only way to get excitement unmarked by a particular agreeable or disagreeable tone,—to have both agreeableness and disagreeableness so suggested that one cannot say the feeling is to be called either. The result was a fall of volume with lower pulse. There was, of course, some attention, but no marked strain. (Pl. 2.)

W 96. Again C, E, G is the stimulus. A surprise, and not especially agreeable. Probably little more present than some excitement. The volume falls with smaller pulse. (Pl. 2.)

B 58. The violet light was on from the start and was turned off at the mark. This gave rise to a depressing, "closing in" feeling,—a somewhat disagreeable depression. The volume fell for a short time. The breath wave in the volume shows throughout. The subject was merely resting before. (Pl. 3.)

P 111. The stimulus was an unexpected noise. The subject was startled. The experience was disagreeable and exciting, perhaps some strain later. The fall of volume was marked. (Pl. 3.)

B 28. Noise. Shock which lasted almost all through, a sense of disagreeable excitement and strain resulting. There is a temporary check in the breathing; a suggestion of rise in the volume curve just preceding the marked fall. In this case the rise may be no more than a broken breath wave, but we shall see later a meaning which such a rise might have. It is worth noting here, however, that there is often no suggestion of it in the shock tracings from the finger plethysmograph. The breath wave in the normal pulse is crushed out in the reaction. It should be stated that this subject almost always shows more or less breath wave in the pulse, although under no strain or abnormal mood. Different persons show great variations in this respect. (Pl. 4.)

It is useless to reproduce more of the results here, especially as almost every condition will be illustrated later in studying the changes in peripheral and cerebral circulation. I shall pass now to a treatment in tabular form.

Those cases were omitted which showed no determinable reaction, as well as those which could not, on the basis of the introspections, be classified. About one hundred and fifty were thus retained out of four hundred and fifty experiments made. To tabulate the results, columns were ruled upon blank paper for the following headings and in the order given: Number of the Experiment, Stimulus, Introspection of Conscious Condition, Volume Change, Wave in Volume, Height of Pulse, Depth of Breathing, Remarks. Each of the usable results was then carefully studied and recorded under each heading. The introspection was analyzed and stated in terms of agreeableness-disagreeableness, strain-relaxation, excitement-depression. In the volume column, C is used for fall, R for rise and Sr a secondary rise which quickly changes to a fall of volume. Under the heading "Wave in Volume," B is used for breath wave and T. H. for the Traube-Hering wave, or perhaps better called Mayer wave. + is "present" and "—" absent in the succession normal, reaction, recovery. When either period is not long enough to determine the character of the wave a "o" is entered in its position. ± signifies a stronger wave and ‡ a much stronger wave than is found in another period marked by a +. A —— in the wave column means that there is no wave that can be followed. In another column it signifies "no change." These abbreviations will also be used to tabulate the heart and breathing rate changes later.

I shall now give a few of the detailed analyses in the following table (Table I).

U 40	Violet.	Agreeable.	C.	T. H. +++	Sl. decrease.	Increase.	
W 109	Cinnamon.	Agreeable.	Sr. then C.	T. H. +++	Decrease.	Inc. marked.	
W 112	C. E. G.	Agreeable.	C.	T. H. +−−	Decrease.	Decrease.	Little fall from the suspense at sudden stopping.
B 80	Discord (announced before).	Some surprise at announcement. Disagreeable.	C. at ann'ncement and at discord.	B. +−+	Decrease.	——	
B 10	Caproic acid (announced before).	Disagreeable.	C.	B. +−−	Decrease.	——	
W 128	Multiplying.	Strain.	C.	——	Decrease.	Irregular.	
B 22	Drawing on face (ann'ced before).	Strain.	C.	B. +−−	Decrease.	Decrease.	
S 107	Violet light.	Excitement. Agreeable.	C.	——	Decrease.	Increase.	
P 150	Blue light.	Agreeable. Exciting.	C.	——	Decrease.	Slight decrease.	
P 141	High whistle.	Excitement. Disagreeable.	C.	T. H. +−−	——	——	
W 79	Noise.	Disagreeable. Exciting. Strain?	C.	——	Decrease.	Increase.	Catch in breathing.
K 8	Drawing on face (ann'ced before).	Announcem'nt caused *Spannung*. Strain at drawing.	C. in each case.	T. H. ++−	Decrease in each case.	Decrease.	
P 128	Yellow light.	Pleasant. Depres'ng and disagreeable when turned off.	C. with light and also when turned off.	——	Decrease when turned on and when turned off.	Slight decrease.	
B 79	Sorrow by suggestion.	Disagreeable and depressing.	C. marked and quite irregular.	B. −+−	Decrease.	Increase.	Breathing slowed.

The expression "Drawing on face" means that a figure was drawn which the subject should try to recognize. The stimulus in B 79 was by suggesting a very sorrowful event in the subject's experience. It will be noticed that P found a blue light more exciting than depressing.

We may now summarize the volume changes as follows, having regard to all the results retained as of value. Twenty-two cases of strain gave a fall of volume. Relaxation gave a rise. There may be a temporary fall at the first moment of relaxation. Eleven cases pronounced distinctly agreeable with no other feeling to be noted gave fall of volume, generally quite marked. Five agreeable gave a preliminary rise followed by a fall, in three of which the fall was much more marked than the rise. There was a rise with two others. Eighteen agreeably exciting caused a fall, and one a preliminary rise followed by a fall. (P 145.) Eight merely disagreeable gave a fall and one a rise. Five cases of excitement were accompanied by constriction. There was a fall with two depressing and four rather disagreeably depressing. Over fifty curves, mixtures of disagreeableness, excitement, and strain in nearly all proportions,—all gave a fall of volume, generally very large. Two indifferent stimuli caused a rise by comparative relaxation from a preceding strain.

It will be seen that the reaction is almost universally a decrease of the volume. It is probable that none of the rises were due directly to the stimulus. Six of those with agreeable conditions were with smells in which the amplitude of breathing is always more or less, generally greatly, increased. The first effect of deeper breathing is always a temporary rise. The other rise with agreeableness comes when the subject had been thinking intently upon another problem just before, and the stimulus, a chord, comes as a comparative relaxation. The rise with agreable excitement we have already seen is probably a Traube-Hering wave, and that with disagreeableness is about like it.

The importance of the Traube-Hering wave in confusing results is not generally appreciated. It is present in the charts published in the literature more than is usually recognized. There is probably no time when some considerable trace of it may not be obtained with fairly delicate apparatus. Far from being absent in deep sleep, it is more important then than in an indifferent waking state. It is often very prominent in sleep when the subject can remember of no dream even if waked immediately. I have easily traced it through several hours of deep sleep with practically continuous records, in fact through the whole night's rest. To be sure, it is often more prominent in the volume in a condition of drowsiness, particularly just before sleep, but not so marked after a long sleep. Even then it may not be so prominent in the other functions and sometimes the largest waves are found in sleep. It is not due to vague conscious states, but is a physiological rhythm probably of the vasomotor centres and secondarily modified by a rhythm in the heart rate.

The size of the pulse in these curves always decreases with fall of volume and increases with rise of volume. This, of course, makes no test of the changes in the heart beat directly, —the vasomotor changes tend to obscure any variations in the force of the heart action itself.

On many of the records, the waves in the volume were not marked enough to determine their changes. The only correlations that could be worked out are as follows: With strain (close attention) there is a tendency to crush out the breath wave, and perhaps also to some extent with disagreeable smells. The Traube-Hering waves tend to become somewhat less with disagreeably exciting (five decreased, to twelve unchanged) and with agreeable and agreeably exciting stimuli.

A study of the results under the heading "Depth of breathing" showed some points of importance, all of them, however, more or less commonplace. Agreeable smells deepen the breathing and disagreeable and disagreeably exciting make it shallow. A disagreeably exciting sound or a noise tends to deepen breathing and often makes it irregular also. Agreeably exciting stimuli at least as often increase as decrease the depth. Attention to multiplying or similar work gives a decrease more often than an increase, but the decrease is comparatively small. By all means the most marked lessening is with attention to sensory stimuli, as in counting marks or listening to a liminal sound.

A phenomenon of some importance for a theory of agreeableness-disagreeableness is the effect of an abrupt stop. Subjects often stated that the sudden muffling of the tuning forks left them "in the air," lost for a moment. This is much more marked if an agreeable chord is stopped suddenly than if we are dealing with a disagreeable discord, although there is no thought in either case of "wanting to stop it." A sudden interruption of a simple agreeable experience seems to be more felt as a change, a shock, than a like interruption of a disagreeable, and in two or three cases a noticeable fall of volume accompanied this shock with the interrupted chord.

The only instances which gave a rise of volume that seemed to be due directly to the stimulus conditions were with attention to the member in the plethysmograph, the finger, to count the pulse and note any other sensations that may come from it. The condition is one of strain only. There is here, too, a tendency to constriction, but also apparently a tendency which may cause the volume with or without a temporary fall to rise with increased height of pulse considerably above the normal and remain there until attention changes. The breathing and probably heart rate changes are the same as with tactual attention generally. The following instance may be given.

B 65. Attention to finger. No particular surprise, a little when asked to cease attending. Some difficulty was experienced to put attention on the finger. There is a preliminary fall followed by a more marked rise, with a return to normal afterward. The depth of breathing is somewhat decreased. (Pl. 4.) Four cases gave a rise without previous constriction.

THE VOLUME OF THE BRAIN AND OF THE PERIPHERY.

The subject for these experiments was a young man, Carl Jahnke, a laborer of fully average intelligence. About two and one-half years before this work was begun he met with an accident which necessitated removal of a piece of the skull, roughly eight by six cms. in area on the right side of the head near the Rolandic region. Motor control in the left forearm was injured for a time. The wound had healed well and the patient had worked in Ann Arbor over two years without inconvenience. The hair is rather thinner on the scalp covering the opening, and there is a considerable dip or hollow at that place. The scalp forming the floor of this dip can be felt to pulsate.

The peripheral volume was taken from the hand in all the experiments on waking reactions to stimuli. A Hallion-Comte plethysmograph was used and connected to a piston recorder. The breath was recorded with pneumograph and Marey tambour as before. To get the change in volume of the brain, the capsule of a tambour was taken and the top covered with thin rubber. A piece of cork cut to fit the dip over the trephine was fastened to this rubber by means of beeswax. A strong cloth bandage was then tied firmly around the head, covering the forehead and the occipital protuberance. From this another broad bandage was passed over the trephine to the opposite side. The hair was parted away from the dip (shaving the head was entirely unnecessary), the capsule inverted and the rounded cork placed in the cavity. This done, the broad top bandage was pulled firmly but not too forcefully down upon the instrument and tied. The capsule was then connected by a flexible rubber tube to a delicate piston recorder. The kymograph employed in most of the work was the Zimmermann model mentioned above, and in a large part a long extension paper was used.

The subject was always in one dark room and the recording apparatus in the other as before. In the experiments here reported he was comfortably seated at a table next the wall. The stimuli given were like those used in studying the volume changes described above, and, in addition, a secondary current was, in several tests, passed through the free hand of the subject. An exposed wire was, in these cases, employed for one electrode. This caused strong cutaneous pain with very little, generally no, contraction of the muscles of the hand and arm. Another person usually assisted me in giving many of the stimuli.

The introspections were taken as before. The subject was asked to describe his experience as fully as possible. He, of course, knew nothing of the tridimensional theory of feeling, but a little judicious questioning in addition to his description would bring out with practical certainty the character of the mental process. Naturally he was, at first, rather nervous, in a state of strain and uncertainty, and the results so far obscured. But it was not long until he became acquainted with the apparatus and methods so that the work became a matter of course. Hundreds of records have been taken from him during the past year almost as a part of his regular routine.

It may be noted here that the circulation in the scalp is negligible so far as its influence on the brain record is concerned. A chart will be given with a plethysmographic curve from the scalp and this will perhaps be sufficient to show the point. Movement also plays no important part in the tracings expressing the waking reactions to sensory stimuli. This is indicated by a consideration of the same curve. It was practically impossible to move the scalp in a way that would affect the trephine without interfering almost as markedly with the region of the bandages which always moved with the scalp. And any change in the bandages must have shown itself by raising or lowering the pressure in the tambour transmitting the pulse from the scalp at least as much as in the other tambour, since this one was over bone. And indeed it did in other experiments. But no such changes were noted in the curves showing the scalp pulse, although types of the only reactions in which movement could be suspected were obtained there. Furthermore, when movement is present its results are easily recognized. Several tests were made in which the scalp was moved in known ways by another person as also voluntarily (so far as he could) by the subject himself; and the effects are readily distinguished by their abrupt character and the kind of break they make in the pulse beats with which they start.

I pass now to a description of a few of the individual results. The chest breathing tracing is just below the indicator line; next is the plethysmographic curve from the hand; and below that the brain record.

No. 39. The subject was asked to attend to a faint watch tick. In a normal condition before. Considerable effort was required. Relaxed afterward. The volume of the hand fell with decreased and rounded pulse. There was a secondary fall at the first period of relaxation after which the curve returned to the normal. The brain volume rose gradually with higher pulse, then returned regularly to normal; the dicrotic was relatively more elevated in its position on the pulse beat, and a little sharper. The breathing was rather shallower. (Pl. 5.)

No. 42. Assistant drew a figure on the hand of the subject, who, with eyes closed, was to attend to and determine the figure if possible. No surprise. Could not tell what figure was drawn. The condition was primarily strain of attention. The amplitude of breathing was somewhat decreased. The volume of the hand fell with more rounded pulse. The brain changed about as in No. 39. (Pl. 6.)

In this and all other charts the mark "art" signifies that the change was made artificially by means of the piston used to regulate the height of the needle of the piston recorder.

No. 40. (The curve is not reproduced.) A series of figures to be added, divided, etc., was read to the subject, who was asked to perform the task suggested. He, with closed eyes, followed the problem without confusion, except a little at the last. The amplitude of breathing is increased. The volume of the hand was decreased as usual. The brain volume rose a little quickly, then fell back and increased gradually to its greatest volume near the end. It then began its return to normal. The dicrotic in the brain pulse was higher in its position on the beat.

No. 88. Only the end of this experiment is given. The subject had been attending to a faint sound for some time, and ceased attending at the upward movement of the marker. There was no surprise when the signal to stop was given, but there was a marked feeling of relief. During the first period of relaxation there was a fall of the hand and rise of the brain volume, after which each returned to normal. There was but little change in the breathing. (Pl. 7.)

Such alterations in the brain volume are often as great, sometimes greater than in this curve.

No. 69. At a signal from the operator, the subject took chocolate into the mouth. The signal had been prearranged, so that there was some expectation of its coming, still there was a little surprise. The taste was agreeable. In the movement of opening the mouth, etc., the bandages were apparently disturbed somewhat, giving a rise in the brain curve which seems to be at first a combination of movement and vasomotor change. The volume and size of the pulse of the brain were increased with more marked undulations then gradually decreased. The volume of the hand fell. The breath was little disturbed except by taking the chocolate and occasional movements of swallowing. (Pl. 8.)

No. 144. Chocolate was again the stimulus. The subject was normal before and took the chocolate with less movement of the mouth. The taste was very agreeable. The volume of the brain increased with greater pulse and the dicrotic a little nearer the apex. The hand decreased with smaller pulse. The breathing was little changed except by swallowing at times. (Pl. 9.)

No. 73. The stimulus was quinine. The subject expected the signal, and there was but little movement in taking the substance into the mouth. The taste was very disagreeable. The volume of the hand fell markedly with low pulse; that of the brain rose with higher pulse and gradually returned to normal. The dicrotic in the brain became higher in position and less prominent. The breathing was a little uneven. (Pl. 10.)

No. 61. The stimulus was violet light. It was markedly pleasant and stimulating. The volume of brain and hand, were nearly, although perhaps not quite, recovered from the effects of the preceding test; that of the hand fell with the light, that of the brain rose, and both then gradually returned to normal. |The dicrotic in the brain was sharp throughout. The breathing curve was interfered with by the fact that the needle of the tambour was striking against the electric marker at the top. The breathing was somewhat deepened at first by the light. (Pl. 11.)

No. 55. The stimulus was a chord. It suggested some church music to the subject. It was agreeable and somewhat arousing. The volume of the hand fell with decreased pulse, that of the brain rose with increased pulse as usual. The amplitude of breathing was decreased at first. (Pl. 12.)

No. 60. The white light was turned out at the marks. This caused the subject to feel depressed for a time. Again there was fall in volume of the hand and slight rise in that of the brain. (Pl. 11.)

No. 78. The stimulus in this experiment was an unexpected deep whistle which greatly frightened the subject. There was a temporary rise in the volume of the hand, but with decreased pulse. This was followed by a marked fall. The brain volume rose greatly with increased pulse, then fell nearly to the normal temporarily, finally rose again and gradually returned toward the normal later. The dicrotic was less prominent but no higher in position. This behavior is quite characteristic of all intense stimuli; and it may be noted that there is often some trace of it in part of the curves given above. (Pl. 13.)

No. 92. A chair loaded with weights was tipped over behind the subject seated in the dark room. Subject reported that he was "scared to death." Could not make out what was the trouble. It was a considerable time before he could collect himself again. The breathing was checked at first, shallowed and showed a tendency to inspiration, after which it became deeper. The volume of the hand rose a little with decreased pulse, then quickly fell so markedly that the needle had to be raised artificially several times. The brain volume rose with increased amplitude of pulse beat, then fell with an undulation to normal. It will be noted that the pulse beat at the time of this fall was smaller and more rounded. With the later fall in this same record it is the nearest approach to a tricuspid pulse that I have found in the brain, except in one curve to be given later. The slightly greater pressure of the needle upon the drum may be responsible for this, but it certainly suggests a hampered pulse. After this temporary fall, the brain volume rose so greatly that the needle of the piston recorder stuck at its base and could rise no higher, so that the full form of the pulse is not shown. The amplitude of the beat was enormous. Finally there was a quick fall with somewhat rounded pulse, and the needle had to be raised artificially. (Pl. 14.)

It is to be regretted that the record was somewhat marred in removing the long paper. There was apparently a slight leak in some of the tubes connected with the brain recorder.

No. 143. A whistle was blown loudly near the subject in the dark room. It caused a marked shock and jump, fright, which persisted for a considerable time. There was irregularity due to movement in the breathing curve, with a decided tendency to deeper inspiration, and fall of its level. The hand volume was so obscured at first by the movement that one cannot determine with certainty the changes which took place, but there was probably a preliminary rise with small pulse followed by a marked fall. The brain increased in volume greatly with high pulse, then fell with lower and somewhat restricted pulse, and, after a smaller undulation, rose again with enormous pulse, and finally returned gradually towards normal, with dicrotic nearer the apex than in the normal. (Pl. 15.)

No. 98. The stimulus was a secondary circuit through the free hand of the subject; an exposed wire was used as one electrode. It did not pain much at first, but became very strong later. It was then very disagreeable, but did not cause contraction of the muscles of the hand and arm. The breathing became irregular, with no distinct check in it, the level of its curve fell. The hand volume increased at first with smaller pulse, then decreased markedly. The brain showed a temporary rise of volume corresponding to the rise in the hand, followed by a fall, after which the second rise appeared. The hand then constricted again more markedly, probably as the current became stronger. The brain increased greatly, decreased and increased again just after the stimulus ceased. The subject expected another shock afterward. The volume gradually returned to normal. (Pl. 16.)

No. 141. The stimulus was again an electric current. It was strong and painful throughout, although not without some variations in the intensity. It did not cause contraction of the muscles. The breathing was deeper. The volume of the hand fell from the start with restricted pulse, and was raised several times artificially. The volume of the brain rose with two waves, a further short rise occurred near the end of the stimulus, after which it slowly returned to normal. The dicrotic moved nearer the top of the brain pulse wave during the stimulus. (Pl. 17.)

A check in the revolution of the drum caused a deformed curve a short distance before the end of the stimulus.

No. 157. Several experiments were made to test the part which the circulation in the scalp might play, as well as to determine further the effects of any possible movement, and this record has been selected to represent the reactions. A second pneumograph was used upon the abdomen. This recorded by means of a Marey tambour the curve above the electric marker line. The plethysmographic curve from the scalp was recorded in place of the volume of the hand. A second instrument, like that used over the trephine to transmit the changes of the brain to the recorder except that the cork used was flatter, was placed in an analogous position under the same bandage on the other side of the head. It was connected to the piston recorder before employed to write the hand changes. The circulatory variations in an area of scalp similar to that affecting the brain curve were thus recorded. Any movement of the bandage, which practically moved with the scalp, must affect this second instrument even more than that over the trephine, since the bone under the scalp would cause every change of position to be fully transmitted to the recorder.

The stimulus for this record was a low, grating noise made at the door of the dark room in which the subject was seated (the mark is a little late). It caused marked fright which did not entirely cease until the drum stopped. There was very little check in either the abdominal or the chest breathing. The volume of both brain and scalp increased with higher pulse, then decreased with smaller pulse, and lastly repeated this process a second time. The changes in the two curves go together. The pulse was almost anacrotic throughout. (Pl. 18.)

About one hundred and fifty curves showed clear results. In general, all agreeable or disagreeable stimuli, all sensory attention or attention to arithmetical problems, all agreeably exciting light or music gave a fall of volume of the hand with smaller pulse and more rounded dicrotic, and rise of volume of the brain with larger pulse, often a dicrotic raised in position and made sharper. A sorrowful condition as well as the depression from sudden darkness produced the same result. The strain of expectation when a neutrally toned stimulus was announced before it was given acted like any other attention—it increased the volume of the brain and size of the brain pulse, and decreased the hand volume and size of pulse. With relaxation there was quite often a fall of the hand volume and rise of that of the brain during the first period, a testimony to the importance of the adjustment period generally; after that a gradual increase of the hand and decrease of the brain to normal.

The first point to be noticed in studying these volume changes is the double character of the reaction, particularly with strong stimuli, fear, etc. The volume of the hand increases first with smaller pulse and then falls quickly to a much lower level with rounded pulse; that of the brain increases with large pulse at first, then decreases nearer or quite to normal and sometimes shows almost an anacrotic pulse, then finally rises markedly with high pulse and gradually returns to normal. There may be even further secondary undulations.

If you ask as to the cause of these changes, several possible reasons suggest themselves. In the first place, it may be thought that the sudden contraction of the muscles of the abdomen, as when one braces one's self for an effort, would force the blood from the visceral organs into the brain and peripheral parts, and that with the release of this tension one gets the secondary fall of the brain. But a voluntary attempt to expire, when the nose was held closed, gave no such quick, large rise as we find here. Again the breathing change is not nearly long enough to account for the rise which may cover several breaths; and in addition the main tendency is to inspiration modified by the irregularities of movement. This type of reaction also occurs in greater or less degree when there is no appreciable check in the breathing, as in some cases of fright and almost always with the painful electric shock. And in the latter case there was also no considerable contraction of muscles which might influence the reaction by compressing the veins.

The only other probable explanation of the preliminary rise is to ascribe it to a sudden violent contraction of the splanchnic vessels. For the change occurs in the brain, hand and scalp at the same time. The smaller size of the pulse in the hand may be due to the marked tendency of the arteries there to constrict also. Even then there are at least two possible explanations of the secondary fall in the brain. It is quite probable that the spastic constriction of the splanchnic is followed by as sudden dilation, or there may be more than one such wave. The second more lasting rise of brain volume follows from the general constriction and consequent rise of pressure. This explanation is indicated by the fact that the later as well as the earlier changes of the brain and scalp occur together. It is possible, too, that there is a local contraction of the brain vessels themselves which is afterward overpowered by the rise with the general reaction. Such a possibility is suggested by the more nearly tricuspid form of pulse often occurring during this period and later. It may very well be that such local vasomotor control is active, although it is to be expected that it would not be sufficient alone to account for the decrease of volume. To decide the question, if possible, I expect to test other aspects of this reaction soon, especially as it appears in blood pressure measurements and in the rate of transmission of the pulse wave.

Another factor that may play some part in these volume changes is the variation of heart rate with strong stimuli which we shall study later. The waves in the rate, however, are of shorter duration and relatively little marked. Even the secondary decrease in rate below normal cannot explain the fall to normal or lower in brain volume, if we suppose it is working against a constriction of the arteries. For it does not occur in a majority of cases, and is generally small when it does appear, while there is practically always a decided volume reaction of this character.

A careful examination of almost any one of the curves will show that the Traube-Hering wave in the volume is quite parallel in brain and hand. A crest in the one corresponds to a crest in the other. The question at once arises whether this wave is here an active phenomenon so far as the brain and periphery are concerned, or whether the changes in each follow passively from the reverse changes in the splanchnic, as was found by Roy and Sherrington to be the case with asphyxiated animals. Here again an attempt will be made to measure the blood pressure changes; the variations in pulse form are doubtful. But I cannot discuss the matter here.

It should be noted also that the waking reactions in the brain volume are different for different positions of the body, particularly when the subject is lying down. These differences and their causes are not important in this connection. I must leave them together with a more detailed study of the Traube-Hering wave, sleep and other more physiological matters for further treatment. In truth, what I have said here of vasomotor control in the brain is practically an extract from a more extended article on the brain circulation to appear in the future.

THE RATE OF THE HEART AND BREATHING.

In the first experiments designed to test the changes in heart rate, use was made of a Sommer sphygmograph on the left wrist. This was connected with a piston recorder writing on the kymograph. A parellel record was taken from a finger of the same hand, using the finger plethysmograph with a piston recorder. But, since it was found that there was no appreciable difference between the heart rate as measured from the plethysmograph curve and as measured from the sphygmograph, the latter was omitted in the later experiments and either a finger or a Hallion-Comte plethysmograph tracing measured for the rate. A pneumograph was applied as usual in the later experiments; no breathing record was taken in the earlier.

A long extension paper was used with the Zimmermann kymograph mentioned above. In a vertical line with the piston recorder needles and the electric marker used to indicate the stimulus, another electric marker wrote the time in one-fiftieths of a second. It was run by a current interrupted by a tuning fork. The drum revolved quite fast, so that the record of the time marker could be easily read. The subject was seated in one dark room, the recording apparatus placed in the other as before.

The subjects were Professor Pillsbury, Mr. Bayley, Mr. Jackson, Mr. Schottstaedt, and Mr. Wright. Mr. Jackson was an advanced student in psychology. Mr. Schottstaedt was just beginning his work in the department.

To study the results, the points at which the upward stroke of the pulse beats began were carefully marked, and lines drawn through these points perpendicularly to the time line. When necessary, correction was made for the displacement due to the movement of the recorder needle in an arc; but this error was usually found so small as to be negligible. The length of each pulse was then counted in one-hundredths of a second, since it was possible without difficulty to estimate as near as one-half the distance between the one-fiftieth of a second marks. The errors are believed to be very small, almost never more than one unit, and such an error is of no importance in the face of the changes which the heart rate shows.

These results were then plotted upon co-ordinate paper with a heavy reference line ruled horizontally through the middle and lighter parallel lines drawn approximately six mm. apart on each side of this. There were similar lighter lines vertically across the paper. The standard line was given the value of either thirty-five, forty, forty-five or fifty one-fiftieths of a second (represented as P 35, P 40, P 45, or P 50 at the left end of the line) and each unit of distance along a line vertical to it means one one-fiftieth of a second. Above is positive, below negative to the standard. Units of distance along it represent pulse beats. Points were placed in the appropriate positions on the vertical lines to mark the length of each pulse in one-hundredths of a second, and a smooth curve drawn through these points. A rise in the curve, therefore, means an increase in the length of pulse, or a slower heart rate; a fall in the curve means a shorter pulse, or a faster heart rate. The points at which stimuli were given or removed were marked with crosses. At times, a few pulses were so obscured that they could not be counted individually with accuracy, although the record as a whole was good. These were sometimes omitted and the number omitted marked in the blank space on the record; more often they were averaged,— the total time divided by the number obscured. The fluctuations in a portion thus averaged are, of course, lost, but the distance is always small. Such parts of the curve were drawn in a dotted instead of a smooth line.

The breathing record was treated similarly. The lines were drawn through points marking the completion of the inspiration, which could be determined fairly easily. The single breaths were then counted in one-fiftieths of a second and the results divided by 2, 3, or 4 as necessary, that they could be recorded on the chart with corresponding pulse record; the plotted lengths must, therefore, be multiplied by this number to give the true length. The point representing a breath was placed on the pulse line, in which the inspiration was completed. A dotted curve was drawn through these points. The value of the standard line for the breath record is represented along with that for the pulse record at the left end of the line.

All results which were introspectively bad, or which for any other reason were unsuccessful, were rejected and not counted and charted.

The charts are themselves sufficient justification of this method of studying the changes in rate, and show the comparative uselessness of any rougher method, such as counting the number of pulse beats in ten seconds. In the first place, there are often shorter temporary reactions whose character can be seen only by such a method. Again, even the general changes are uncertain if studied less accurately. There are large variations from purely physiological causes, as Lombard and Pillsbury have already shown; and the kind of results one seems to get by any method of averages depends far too much upon where in one of these waves one begins his count. If the reaction is not large, almost anything can be proved, or seem to be, by starting at the appropriate point. The true character of the process is seen only in charts of this kind.

I turn now to a detailed study of a few typical charts.

B 24 R. Pl. 19. No stimulus was used, the subject reported the record normal and indifferent throughout. It is given to show the character of the changes which may take place from physiological causes and are not to be ascribed to any accompanying mental process. The Traube-Hering wave and the breath rhythm both show distinctly, and there is at first a small rise, later, a small gradual fall of the general curve. Several normal, indifferent records were taken, all showing similar characters. Of course, one may have a gradual rise or fall at any part instead of the one at the end, the other at the beginning as here. In the light of this, much care is necessary to be certain of any conclusion one would draw as to the reaction with a mental process.

P 32 R. Pl. 19. This shows the effect of a voluntary muscular movement. The subject moved his free hand to his face and held it there a few seconds, then returned to the former position. There was no particular feeling to be noted, only the consciousness of an indifferent movement. The result was a short increase in rate followed by a greater but temporary decrease. The curve then returned to normal and is but little affected by moving the hand down to the table again. There was a fall in the volume record at the time of raising the hand.

P 10 R. Pl. 20. Multiplying. The subject pressed a signal key when the problem was solved. Work done without confusion, required some effort. A condition of strain. Some feeling of relaxation afterward. The pulse rate was increased and recovered before the end. There was apparently a slight increase in the first moment of relaxation.

P 18 R. Pl. 19. Multiplying. It required a good deal of effort all through and the subject got the result just at the end of the record. Feeling of strain. It is to be regretted that several pulses at the beginning of the record could not be counted accurately enough to plot, but it could be determined that all were above 40. The pulse rate was therefore increased considerably, more at the last.

J 2 R. Pl. 21. The subject was asked to attend to a faint telephone snarl to get the attention waves. There was no surprise at signals. The feeling of strain was marked, and there was some relaxation at the end. No other feelings were present. The rate was decreased on the whole. The first large waves were in all probability Traube-Hering waves in the rate, which were partially stopped as the rate was slowed. There was a small increase in rate after attention ceased. The breathing was markedly shallower.

B 4 R. Pl. 20. The subject was asked to count the closely written marks of a time record. Normal before. Considerable effort of attention. Strain was the only feeling, some relaxation afterward. The pulse was slower. There was an increase in rate after attention ceased, and apparently a large wave was coming in as the record stopped.

J 8 R. Pl. 21. The conditions were similar to those of B 4 R. The feeling was strain followed by relaxation. There was a marked temporary increase in the heart rate followed by a more lasting decrease and a gradual increase afterward. The breath wave in the pulse was less marked during the attention. The breathing became more rapid and shallower.

P. 28 R. Pl. 22. A telephone snarl was decreased in intensity until one could hear it at times but not continuously. When the subject was again in a normal, indifferent condition, the kymograph was started, and, at a signal from the operator, he attended closely to the sound, pressing a signal key with a finger of his free hand while the sound was audible, releasing the key when nothing could be heard. As soon as the paper had run around once, the kymograph was stopped and the paper dropped so that another curve could be taken. The subject attended continuously. After a few minutes the drum was again started, and when it was about one-half way around, the subject was signalled to cease attending. The paper was allowed to complete its second revolution. A record was thus obtained of the heart rate at the beginning and end of a long continuous effort of attention. The two periods are marked "1st run" and "2d run." The subject reported that it required a concentrated attention, feeling of strain. There was a feeling of "letting down" relaxation afterward. The pulse length was not changed at first, but increased in the latter part of the first run and still more during the second run. There was a temporary increase of rate at relaxation. The Traube-Hering wave in the rate, present in the normal, was almost crushed out with the increase in heart rate during the attention, and apparently was coming in again as the second record ended.

J 20 R. Pl. 22. The arrangement was similar to that in P 28 R, except that the problem was to differentiate two weak sounds. The subject reported that he was indifferent before the stimulus, there was no surprise at the signals, attention was close, with feeling of strain only, and there was distinct relaxation when signalled to cease attending. The pulse rate was very little changed during the first run, perhaps a little decreased. It was faster in the second run, and became slower during relaxation with one large wave just after the stimulus ceased. The Traube-Hering wave was present throughout, but was most marked in the normal and first few seconds of attention, less prominent later. The breath wave in the pulse was smaller during the stimulus. The rate of breathing was somewhat increased, and the amplitude distinctly decreased, particularly at first. It will be noted that there was a wave in the breath rate which suggests in its period the Traube-Hering wave found in the volume. I shall call this the Traube-Hering wave in the breath henceforth, without here discussing its nature. This wave is large in the normal part of the record under review, but is more or less broken up during the stimulus period.

J 7 R. Pl. 22. This again was a long continuous concentration of attention but this time visual,—the effort was to count the closely written marks of a time record. The subject moved the fingers only a very little to mark the number of marks instead of pressing a signal key. In other respects the experiment was conducted as in the long auditory attention tests. The subject was normal before, and reported no surprise at signals, constant attention with feeling of strain, and relaxation after the signal for the counting to cease. The heart rate was not definitely changed during the first run, but had increased in the second run. There was a temporary increase in rate at the first period of relaxation. The Traube-Hering wave in the heart rate was distinct in the normal and stimulus periods, uncertain during relaxation; the breath wave more prominent in relaxation than elsewhere. The breath rate was greatly increased in the stimulus period, less in the second run. One longer and deeper breath will be noted just before the end of the first run; a larger breath wave in the pulse rate accompanied it. The amplitude of breathing was greatly diminished during attention. Traube-Hering in rate of breathing was largely obscured.

B 17 R. Pl. 23. The subject took chocolate into the mouth with very little movement or disturbance. The taste was very agreeable. There was no strain, excitement or other feeling to mention, simply the agreeable taste. The pulse rate was greatly increased. The Traube-Hering wave present in the normal was less in the first part and disappeared in the last part of the stimulus.

P 48 R. Pl. 23. Chocolate. The subject had held the candy between his teeth in a slightly strained position before; otherwise normal. The taste was agreeable, there was a little relaxation from the slight strain, no excitement. The rate of the heart was increased on the whole and the Traube-Hering wave became more prominent than in the normal. The breathing was very little changed, there was less wave in it in the later period.

J 10 R. Pl. 23. Chocolate. Agreeable. There was no disturbance nor other feeling than agreeableness. The pulse rate was increased and more of an undulation came into the curve. The breathing became faster on the whole, but with enormous waves.

P 49 R. Pl. 24. Quinine was taken into the mouth with very little movement. It was very disagreeable; possibly a little excitement; but that very slight. No other feeling. The pulse rate became much faster. The large Traube-Hering waves present in the normal were almost crushed out during the reaction.

J 3 R. Pl. 24. The stimulus was quinine. There was a little surprise when the signal was given to take the quinine into the mouth. The subject reported that the taste was very disagreeable, and that was all there was to the feeling. The pulse became faster and the Traube-Hering wave in it less marked. The breathing also was quickened, the exact character of the undulation in its curve is uncertain.

J 15 R. Pl. 25. The stimulus was lively music played upon a zither. It began at the cross and lasted the remainder of the record. The first strain was not particularly agreeable because too loud, but later the music was exhilarating (exciting) and agreeable. The pulse rate was increased and the Traube-Hering wave in it gradually eliminated. The breathing became faster, the wave in its curve was of uncertain character. The amplitude of breathing was somewhat less during the stimulus.

P 41 R. Pl. 26. Music, very pleasant and exhilarating (exciting). The pulse rate became faster and the prominent wave in it somewhat less. The breathing was also more rapid during the stimulus. There was a large wave in the breath rate, the relations of which are not very evident.

P 43 R. Pl. 26. The stimulus was again music of an agreeable exciting character. There was possibly a little strain. The pulse was shortened, particularly in the latter part where the agreeable excitement was greatest. The first part of the curve shows what changes may take place without accompanying mental processes that will explain them. The Traube-Hering wave in the heart rate became less prominent during the reaction. The breathing was on the whole somewhat faster with the music.

P 45 R. Pl. 26. Music was again the stimulus, this time pleasant and markedly depressing. The pulse rate was increased, as was also the rate of breathing. The undulation is not very noticeable in eithe r.

J 16 R. Pl. 27. The stimulus was pleasant and depressing music. The pulse rate was faster, the wave in it less marked during the stimulus. The breathing was also more rapid with the music; the Traube-Hering wave in the breath rate is fairly well shown, somewhat less in the reaction.

S 9 R. Pl. 25. A yellow light was turned on at the first mark, off at the second. The subject reported that it was agreeable to a certain extent in contrast with the darkness, but that the color was not agreeable. Its disappearance was hardly noticed. The pulse was slowed and the breath wave in it more prominent. Very few records were taken with this subject. Two like results were obtained from him by use of a pleasant exciting violet light, and one with the depression from turning off a light. No determinable change occurred with a long effort of attention. An increase in rate accompanied one multiplication test. Two strong shocks with an unexpected whistle and one very irritating light gave faster rate. The subject was unaccustomed to this work and almost always reported that he was "expecting the stimulus." Most of the reactions were probably a release from strain, and have little meaning as accompaniments of the mental process coming into an indifferent state. It was perhaps a mistake that few lights were used as stimuli with the other subjects and only three cases were counted. But they were omitted because they had no effect to speak of, only a suggestion of an increased rate in two of the three cases.

P 38 R. Pl. 27. A loud, unexpected whistle was blown. The subject was startled and jumped. It was disagreeable, exciting strain. Recovered very soon. The pulse rate was temporarily increased, then temporarily decreased, after which it returned to normal. There is a large Traube-Hering wave in the latter part of the normal period, and possibly throughout the remainder of the record. The breathing was made more rapid for a short time.

J 21 R. Pl. 28. The stimulus was again an unexpected whistle. It frightened the subject. Excitement, disagreeableness (and strain?). The general result was a faster pulse for a time, the breath wave in it being a little less marked in the reaction. The breathing became faster for about the same period that the heart rate was increased.

B 10 R. Pl. 28. A particularly sorrowful event in the experience of the subject and to which his attention turned easily was suggested. He reported that he was normal before, that a few seconds after the suggestion was made he fell into a distinctly sad mood, and when told to return to his former indifferent attention he could feel his body drooped, one might say relaxed. It was, apparently, a condition of disagreeable depression for the most part, with perhaps a very little strain at the start, although the subject did not give an accurate account of the feeling directions involved. The pulse rate was slightly decreased, but the difference was not enough to have any meaning. The more important point is the marked Traube-Hering wave which came into the curve.

About one hundred and ten records were counted, charted, and then tabulated by the same methods used in the work on volume changes. Such a study of results showed that the strain of expectation tended to increase the pulse rate. Movement in two tests increased, then decreased it. Multiplying, which was accompanied by a feeling of strain only, gave a marked increase in three cases, no decided change in three. With short close auditory attention there was a decrease in two tests; in a third, no change at first, increase in the latter part. Similar visual attention had as its accompaniment a decrease in rate, twice, a temporary increase followed by a decrease, twice. At least one of these increases was probably caused by movement of the subject as he began attending. Long, continuous visual attention gave in three cases a decrease in rate at first, followed by an increase later; in two no change at first, increase in the second part; in one there was no change except a slight increase from a movement at the beginning. Long auditory attention caused no change, once (with subject S, mentioned before); a short increase followed by a decrease in the first part and increase in the second part, once; slower rate at first, faster later, in two instances; and three times, no change at the beginning, quicker beat in the latter part of the first run and in the second run. Of eight results with chocolate, in seven of which agreeableness was pronounced the only feeling to mention, seven gave increased rate, generally very marked; one gave a short decrease followed by a more prominent increase. There were five records of a disagreeable taste, in two of which there was also a little excitement,—all showed more or less marked fall in the curve. There were seven trials of music which according to the subject's introspection were agreeable and exciting. All resulted in a faster rate, generally the increase was very marked. The emotion of joy, which was almost entirely agreeable excitement, was accompanied by a quickened heart in one case, no change in two. Omitting one instance which was obscured by the effects of movement (the subject was not prepared) there are left four cases of agreeably depressing music, in all of which the pulse was shortened. If we except the results with S which were uncertain, the rather disagreeable depression from suddenly turning out a light caused an increase in rate in four tests, no change in one. Four records were taken with the emotion of sorrow, none of which showed a determined change of rate. The same was true of two with the emotion of anger. Eighteen records were retained, in which the stimulus was a loud unexpected whistle. The stimulus induced a shock, disagreeable excitement with more or less strain. Two of these experiments showed no change; eight gave a faster pulse rate; and eight resulted in a temporary increase in rate followed by a decrease which was generally small, and later returned to normal. The main change during the feeling of relaxation was a slowing of the pulse generally, but this slowing was in perhaps half the tests preceded by a temporary quickening in the first moments of the "letting down" feeling.

It will be noted that there were only four conditions under which a slowed pulse was obtained. We have already discussed and rejected as meaningless because complicated by release from nervous strain, the experiments with colored lights when S was subject. A second condition is in relaxation, when the change is little more than a reaction back to normal added to any effects of fatigue. The whistle in the third place caused a secondary slowing. There seems to be a tendency to both acceleration and inhibition with these strong stimuli; and the rule is that the acceleration is stronger at first, the inhibition, a little later and this decrease in rate is generally small, and is often absent. In fact, the effect of such stimuli upon the heart rate is not nearly so marked as might be expected, not so much as that of the more lasting weaker stimuli, and is not at all in proportion to the effect on the volume. Of course the increase in rate may be due either to inhibition of the vagus nucleus of excitation of the accelerator centre.

The fourth type of decrease in heart rate, that with sensory attention, requires more notice. It is not the reaction to the feeling of strain which is so evidently present. For in the first place, this feeling was fully as strong and as pure in the multiplication experiments, and these gave a shortened pulse. Again, in all except two cases (in which there was no change) of longer effort of attention to both auditory and visual stimuli there was an increase in the rate above the normal in the second run, and in several cases this occurred before the end of the first run. And at least five records showed no slowing at first, the quickening was only delayed. These facts suggest that the real tendency of the effort of attention, the strain shall I say, is to shorten the pulse, and that this tendency is held in check at first by some other factor which tends to lengthen the beat. And at least an indication of this second factor is readily found. It is here in sensory attention, particularly in the earlier part, that one finds by far the most effective decrease in amplitude of breathing, and this generally goes with a much smaller increase in the rate of breathing than is caused by many other stimuli; it may even be decreased. The natural results of this both indirectly and directly by the lessening of the cumulative effect of the respiratory centre upon the vagus would be to lengthen the pulse. No experiments were designed to test this consequence of breathing changes directly, but a measurement as careful as possible under the conditions made upon curves taken for another purpose showed such a pulse change.

No difference was found between visual and auditory attention. There was no difference between agreeable and disagreeable conditions.

A few changes in the Traube-Hering wave in the heart rate have some significance. Agreeably exciting and agreeably depressing music, disagreeable tastes and the disagreeably exciting whistle, tended strongly to eliminate the wave, never increased it. Agreeable taste is ambiguous, twice increasing and twice decreasing it. One of these increases with agreeable taste was accompanied by large variations in breathing. Sorrow, with slowed and irregular breathing, exaggerated the Traube-Hering in the pulse. Strain in attention lessened the wave during the stimulus; it usually returned in relaxation, and in some cases was markedly increased then, a phenomenon which may perhaps be connected with the less aerated blood.

The rate of breathing was almost always increased; in smaller degree for sensory attention than for any other stimulus. Indeed, two cases of sensory attention showed a temporary decrease in the breathing rate preceding the increase, one, gave a distinct decrease and one no change. No other condition was accompanied by a decreased rate.

Each of the original records on which the plethysmographic tracing was clear enough to show the volume changes of the Traube-Hering wave was carefully studied and the highest and lowest points of volume marked. The corresponding lines were then designated in the pulse length charts by the abbreviations T= trough and C= crest. These abbreviations were placed under the curve to which they refer. An examination of these results showed that the crest of the volume wave may fall anywhere on the rise of the pulse-length wave, more often near the crest than near the trough; that the trough of the volume wave may fall anywhere on the descent of the pulse-length wave, but still more often near the trough than near the crest. So that the fall of volume will correspond with a part, commonly the smaller part of the rise in the pulse-length curve, and with a part, still more commonly the larger part of the fall of the pulse-length curve. The rise in the volume will correspond with a part, generally the smaller part of the fall of the length curve, and a part, less generally the larger part of the rise in the length curve.

I have spoken above of a Traube-Hering wave in the breath rate. Perhaps it should not be given this name, for I cannot be certain of its relations, and, in any case, it is much obscured by more or less voluntary influences. But such a wave in the breathing is often found during sleep, and this fact naturally suggests that a similar relation may play some part here. I must, however, leave this also for a fuller treatment later.

AN INTROSPECTIVE REPORT AND CONCLUSIONS.

The subjects were, in general, asked to classify the mental states in terms of strain, relaxation, excitement, depression (rest), agreeableness, disagreeableness, rather than to give an account of the more intimate nature of these processes. Yet some effort was made to get at the matter more closely. If it is unsuccessful, it may at least be suggestive, and may emphasize the need of a more careful study of the introspective side.

Strain is described as composed of sensations from the muscles, the backflow from the acting muscles, particularly those of accommodation of a sense organ. But so is excitement also. Possibly other organic sensations are more involved in what is often called a state of excitement. It is reported of a stimulus that it "kept me on edge all over," "seemed to stir every spot of the body." A condition reported as excitement seemed afterward to be "an indefinite, uncertain, muscular strain." "Felt tingling all over." It may go only so far as to "wake one up," give an increased sensation of muscular tonus.

These analyses, particularly of excitement, are made only after the original experience, or as the stimulus is ceasing. So it is reported after a whistle: "Every shiver seemed to rush in at once, then relief." Relaxation after strain was described as "the receding and more or less indefinite reverberation of the feelings during attention." It is useless to quote many of the introspections. In short, both strain and excitement are analyzable into organic, particularly muscular sensation for the most part; of course, the sensations aroused directly by the stimulus, as the noise, are also involved. When so analyzed, they lose their character as feeling, we have something else; but that is another matter. Yet there is a difference. Strain may be imperfectly analyzed and is felt to be so, as long as it is called feeling; but so far as it is analyzed, every part connects with all others. The whole state turns as a sort of system around a central controlling process. Although not to itself, yet to an onlooker it is teleological as a whole.

In excitement, there are several ends, or several may be read into it; several directions of movement of the strain of consciousness, but none clearly defined and dominant. It is not a closed system of association where every part is associated with every other in its place. It is not the rate of succession of mental processes, it is a half fusion of different lines of association that characterizes excitement. Mutual inhibition may be one means of holding a number of such chains imperfectly in consciousness. Obviously there is no sharp line of demarcation between strain and excitement; such intermediate conditions we find often with a high whistle. It is best to describe the matter this way, rather than to say we have here a mixture of the two.

Relaxation seems to be a release from either strain or excitement. We know the difference in the "letting down" feeling in the two cases, because we feel the difference in the states that are lingering. But in either case, the feeling is a returning from the character of sensation one gets from the active muscles to the kind of sensation from the lax muscle, and partakes partly of the nature of both. It is hardly an opposite of either strain or excitement in the way that disagreeableness is the opposite of agreeableness. It originates only in a release, is a secondary instead of a co-ordinate state.

Depression, rest, name feelings that at least at first sight seem to have more varied forms. It is, I think, based upon a different kind of sensory content, that from the quiet muscle, or one easily acting. These sensations lack the peculiar quality which we call strain when it is isolated as a quality (not feeling). A different rhythm at least gives it a different quality. It comes to be associated with, to mean a condition of, little or no motion. But this is a secondary factor. The tendency to such a condition may be felt as resistance or not, according to circumstances. Thus, if I am sleepy and can go directly to sleep as I choose, the feeling is one of rest, no active oppression. But if I am attempting to work, or if a stimulus is acting that tends to excite me in any way,— in either case there is a felt oppression, a shutting down upon the activity. Looked at in this way or thought of simply as rest or quietness, in any case it is as much an opposite of strain as of excitement. We cannot have it simultaneously with either strain or excitement, except when felt as oppression, and continued strain as well as excitement gives rise to depression. Yet it is not an opposite of either in the way that disagreeableness is the opposite of agreeableness,— it is simply different. B's introspections at least, bear me out in this report.

The most important introspection bearing on the agreeable-disagreeable phases of experience was that to interrupt suddenly an agreeable stimulus is felt more as a shock than to interrupt a disagreeable. It suggests a view which we may perhaps express by saying that the stream of consciousness, always has a momentum. One status is always a condition for the becoming of the next, even though the next be like the present. There is a character of expectation, or that which makes expectation active as much in a continuous sensory process as in looking for a change. These are only two special cases. Agreeableness is then success, disagreeableness failure, but not necessarily in moving toward some other end. The process is a measure of the ability of one stage to adequately prepare for the next. Perhaps this says little more than that disagreeableness has more in common with the interruption in mental process, the fact which I have noted above, but it is so far suggestive.

I return now to the experimental results. Whether the above considerations are correct or not, I think I may speak with more confidence of the organic reactions. I find that feelings cannot be classified on the basis of vasomotor and heart rate changes. There is no reverse relation even between the accompaniments of agreeableness and disagreeableness; much less are there three such pairs of reactions. The tridimensional theory would make strain give a lengthened pulse, relaxation a shortened pulse. In these results they give just the reverse, except as the rate with strain is modified by inhibited breathing. Agreeable, agreeably exciting and agreeably depressing states all give distinctly faster pulse and fall of volume. Neither Wundt nor Lehmann can explain this. Wundt would have the exciting and depressing phases produce no change in heart rate, and agreeableness cause a slowed pulse. Hence, in all these three cases, he should find a lengthened pulse beat; and Lehmann's theory calls for the same at least with agreeableness. Both also hold that agreeableness (and Wundt adds excitement) increase the volume,—we find the reverse. And I wish to emphasize also that the brain does not in my results decrease in volume with agreeable stimuli, and the size of the brain pulse does not decrease with disagreeableness. I need not repeat in detail the reactions summarized at the end of each section of the paper. It will be seen, that, if this work is valid, neither Wundt's nor Lehmann's theory can be allowed.

In short, all moderate nervous activity tends to constrict the peripheral vessels and to increase the volume and size of pulse in the brain. All moderate nervous activity likewise increases the heart rate. Strong stimuli cause both an exciting and inhibiting effect, which is seen especially in the heart rate. They also cause a double reaction in the brain. The most marked effects are at changing periods, particularly with an incoming stimulus. Lastly, the activity of any part, or the prominence of sensations from it tends to counteract constriction in that part.

We may explain these results in part, as follows. It is probable that all moderate nervous activity also causes constriction of the splanchnic vessels. Strong stimuli seem to have both an exciting and inhibitory effect upon them. The results in the heart rate may, of course, be due to an effect on both the vagus and exciting centres. The increase in the volume of the brain is probably in part at least due to increased blood pressure from the constriction of the periphery. It is not attention as such that causes the change. It may be simply reflex. It does not seem necessary that the stimulus reach consciousness, contrary to Lehmann's assertion. At any rate, I think this is indicated by experiments during sleep. Local control in the brain at least by constriction is indicated, but I leave this question open for the present. Perhaps the fact that the activity of any part tends to counteract constriction in it is due to the action of vaso-dilator nerves. The circulatory control depends upon physiological processes that vary roughly with what one is doing.

I append here some references bearing directly on the subjects studied, and most of which have been noted in the text.

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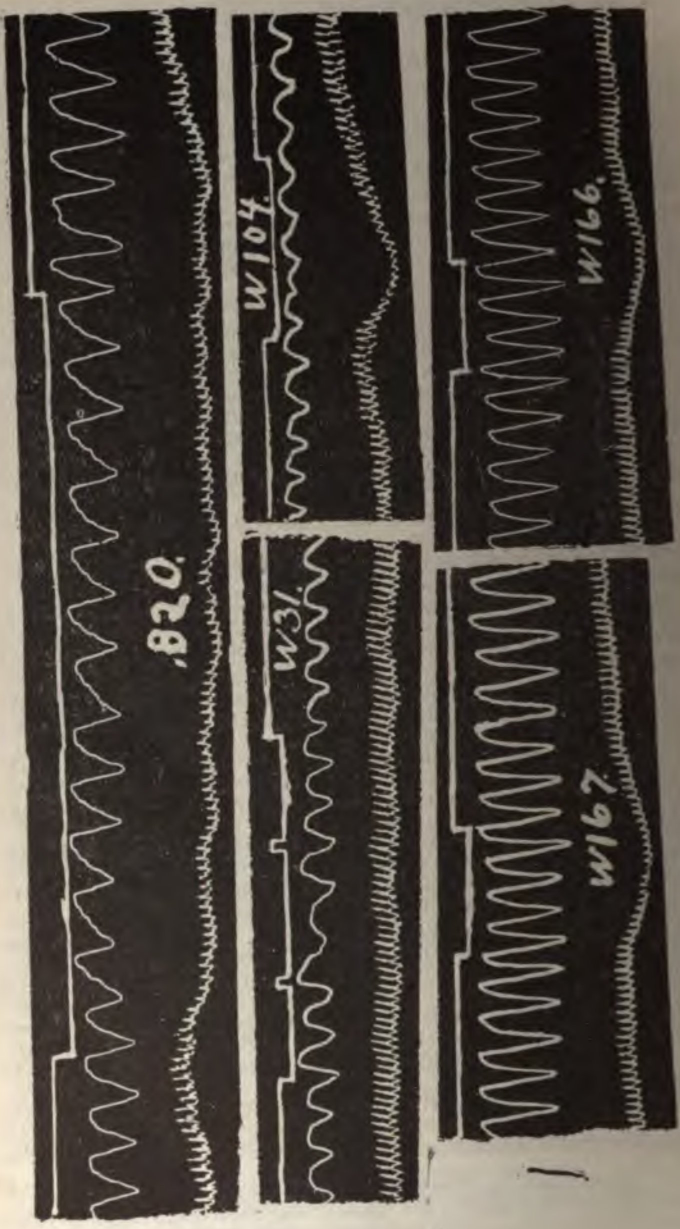

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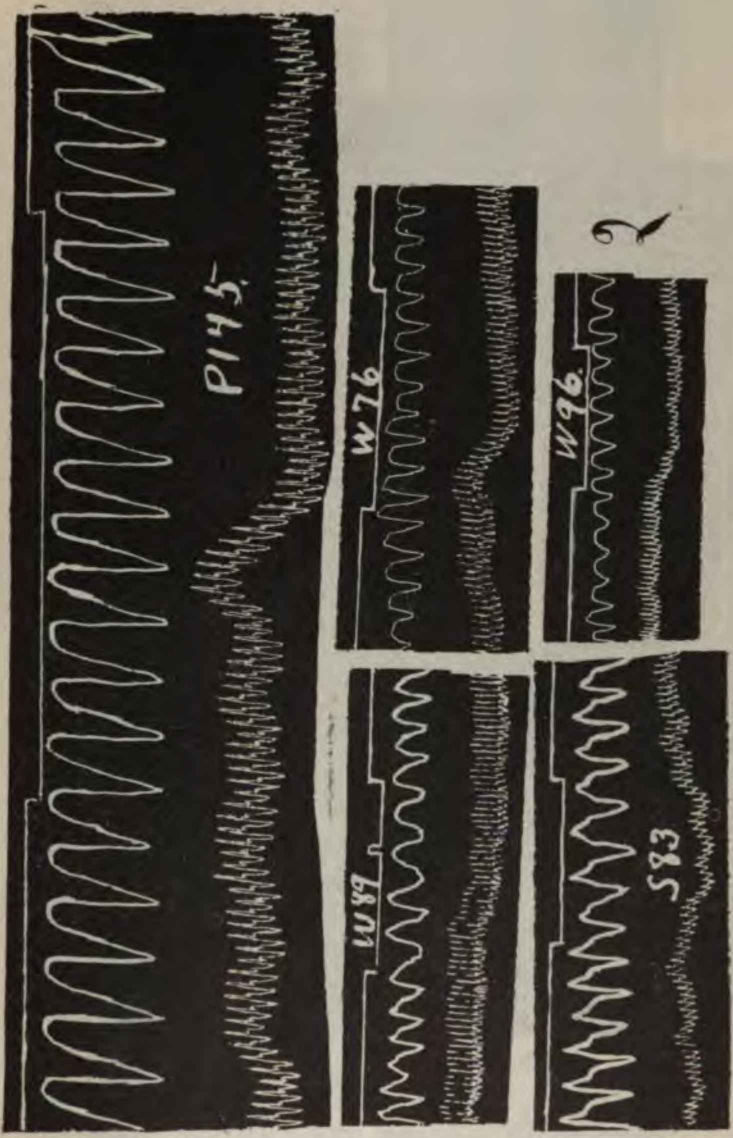

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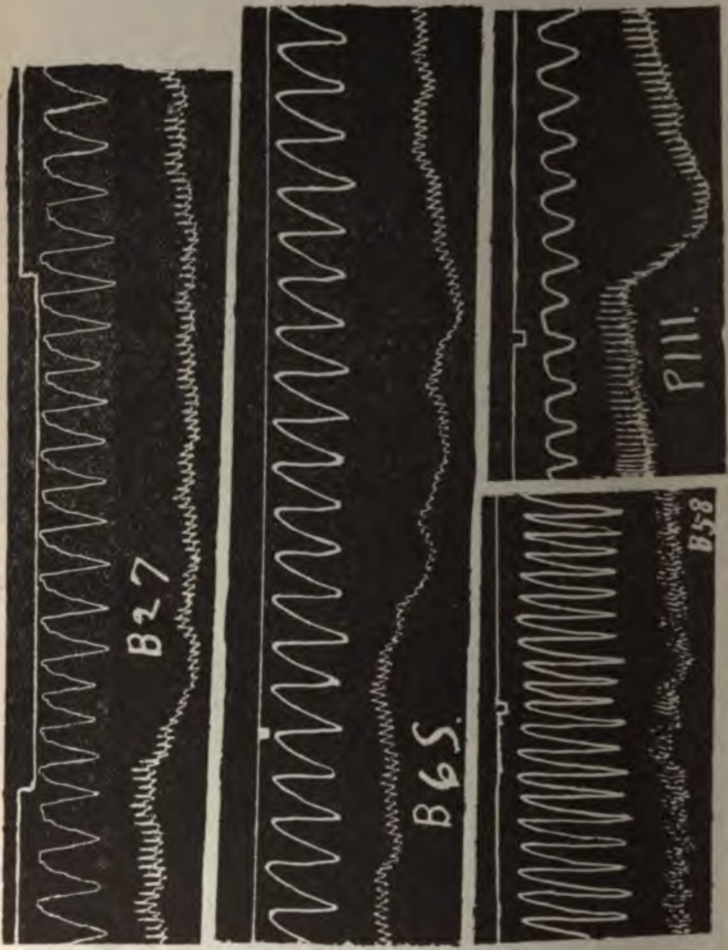

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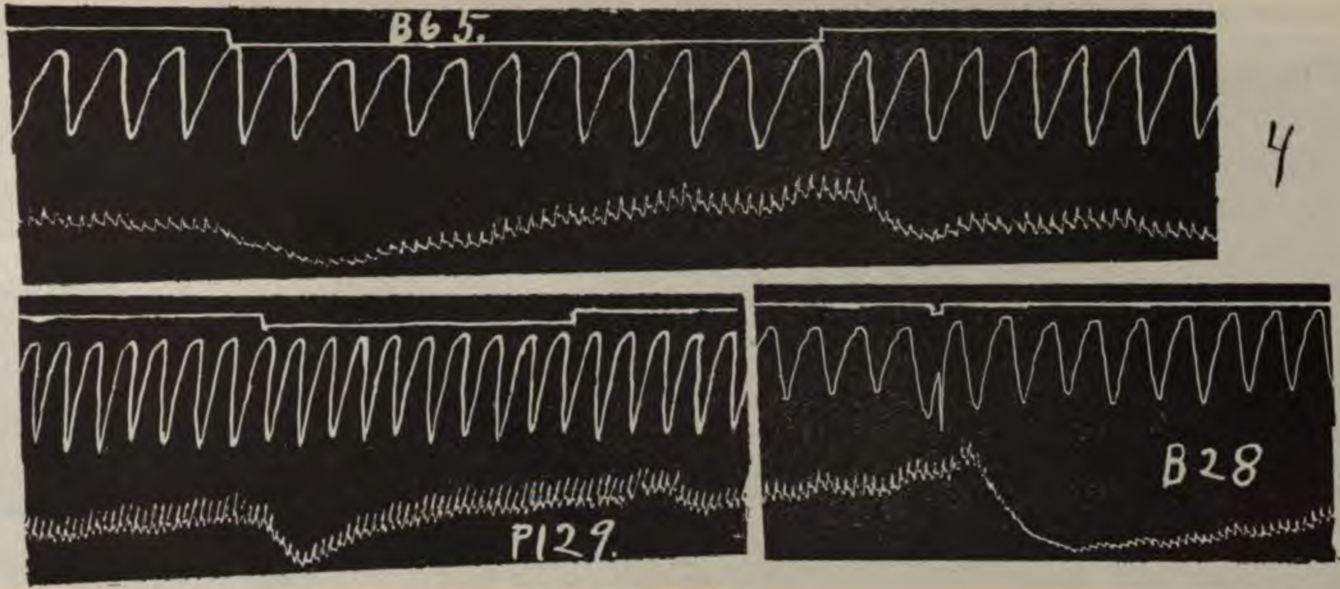

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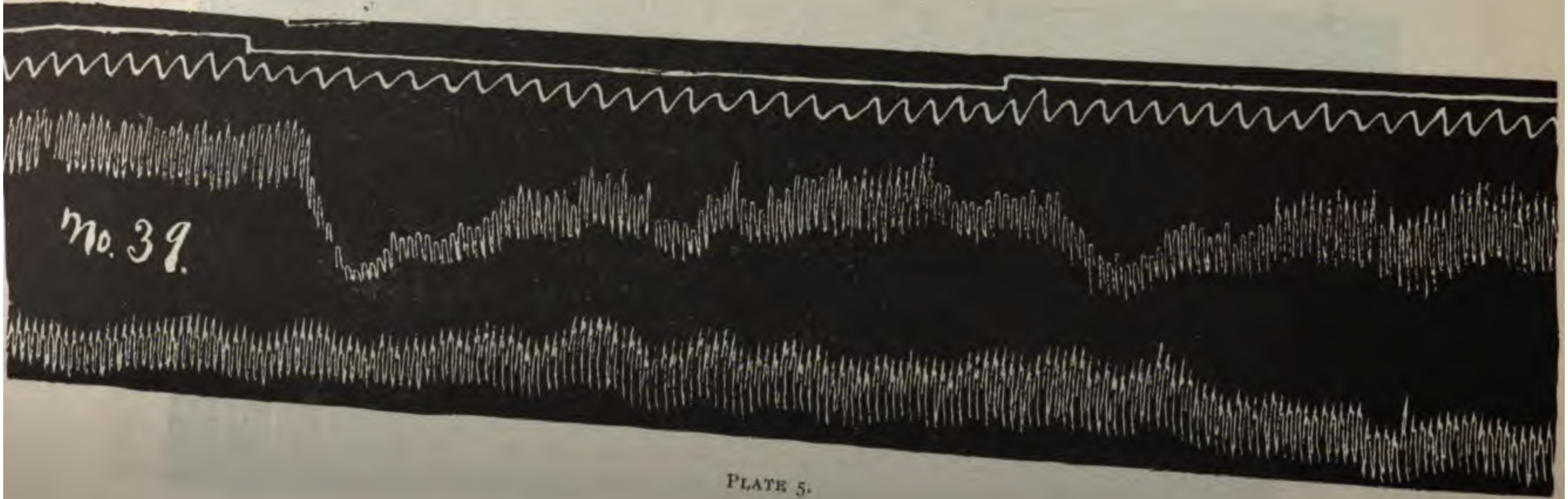

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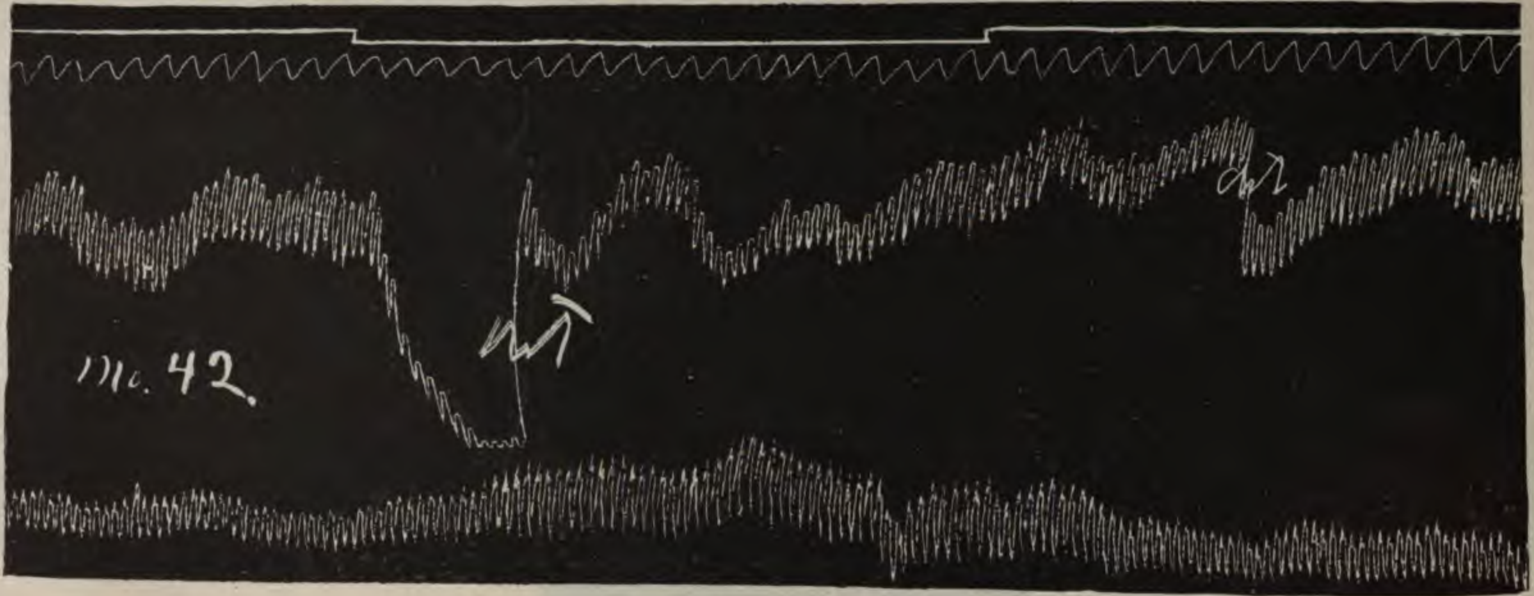

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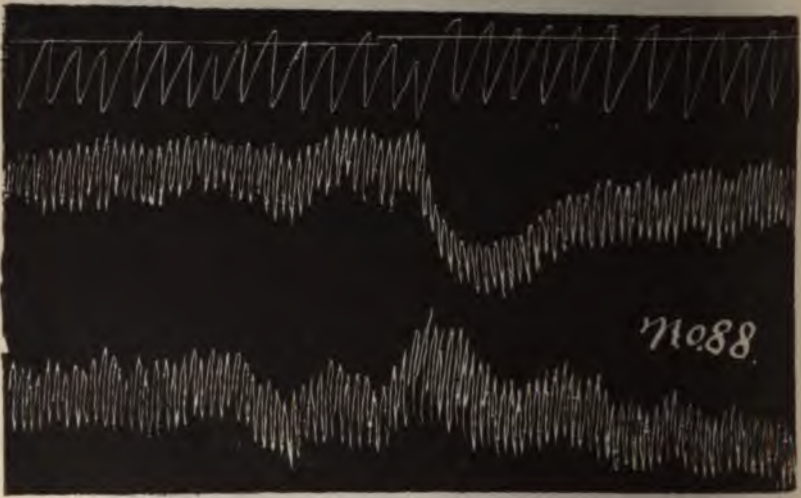

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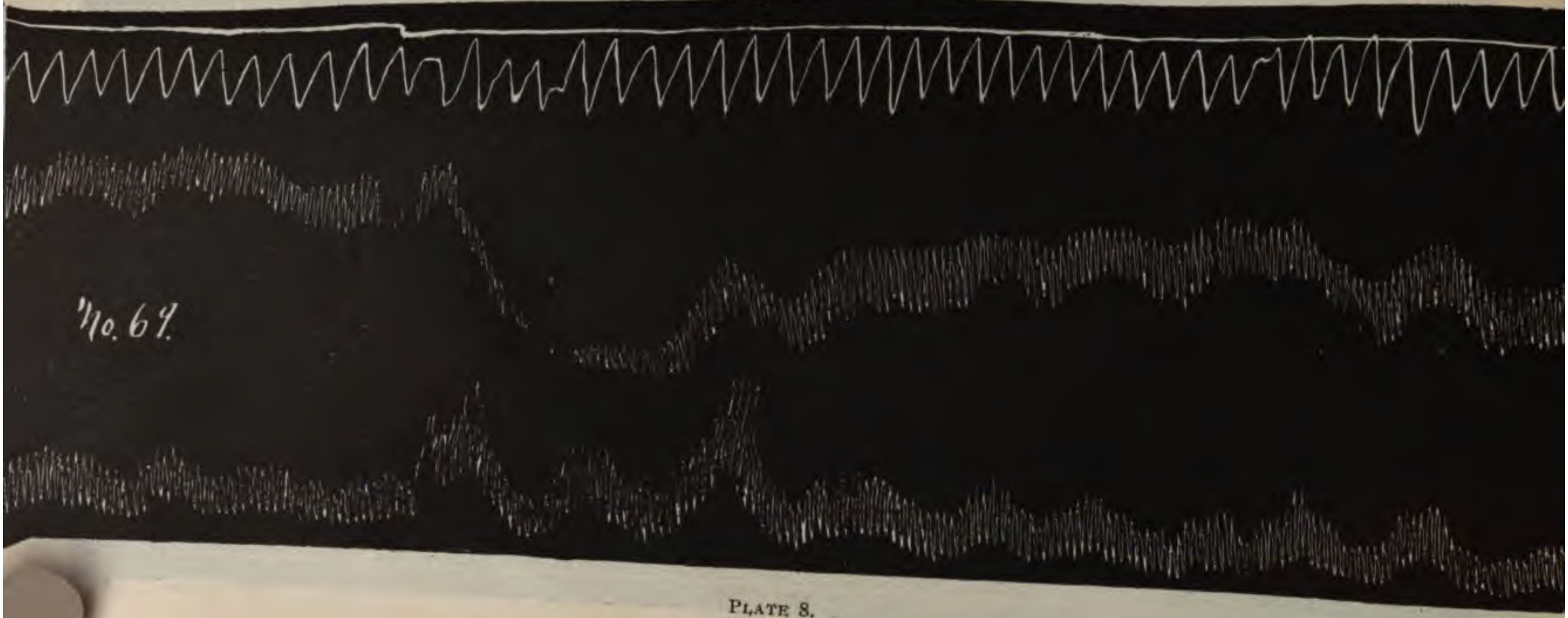

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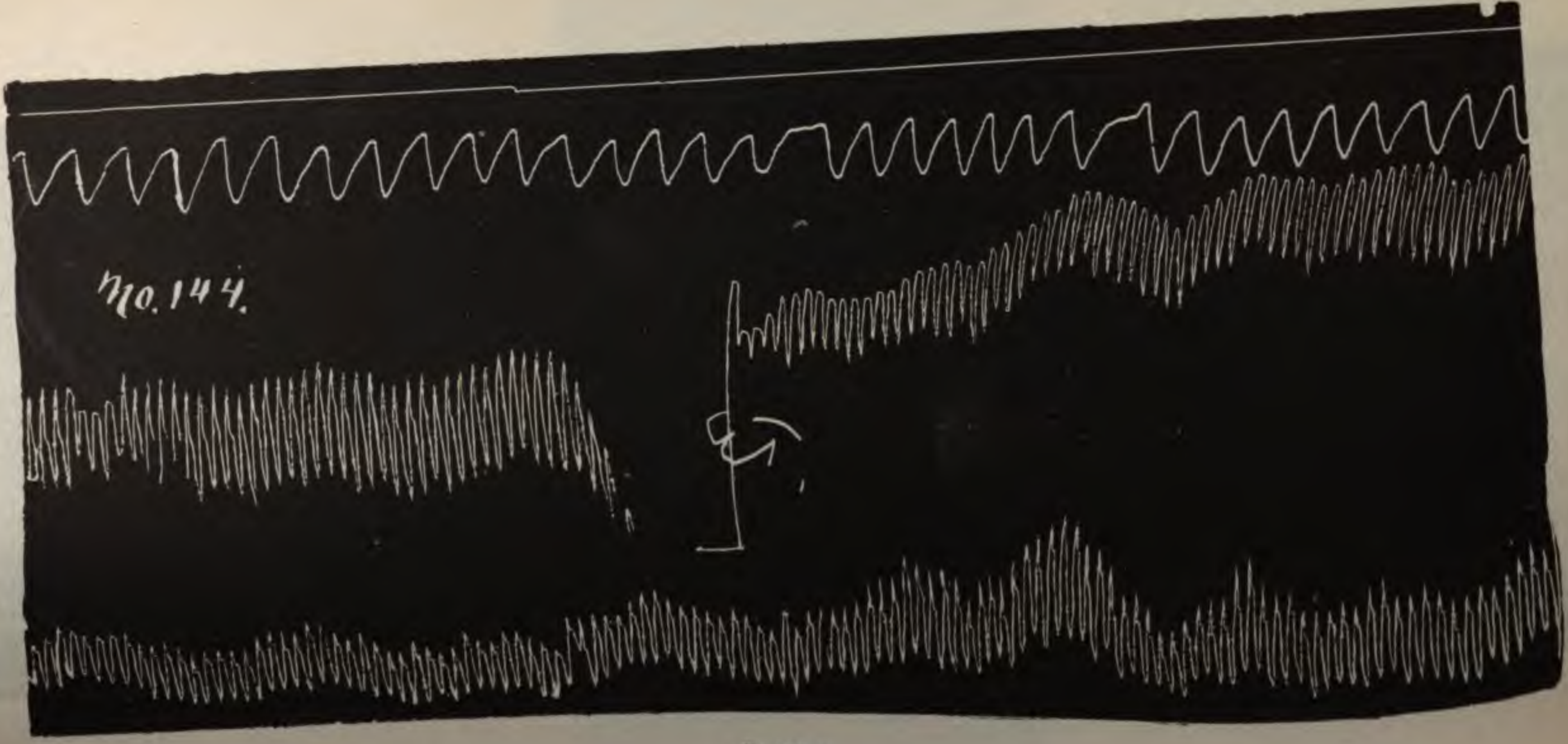

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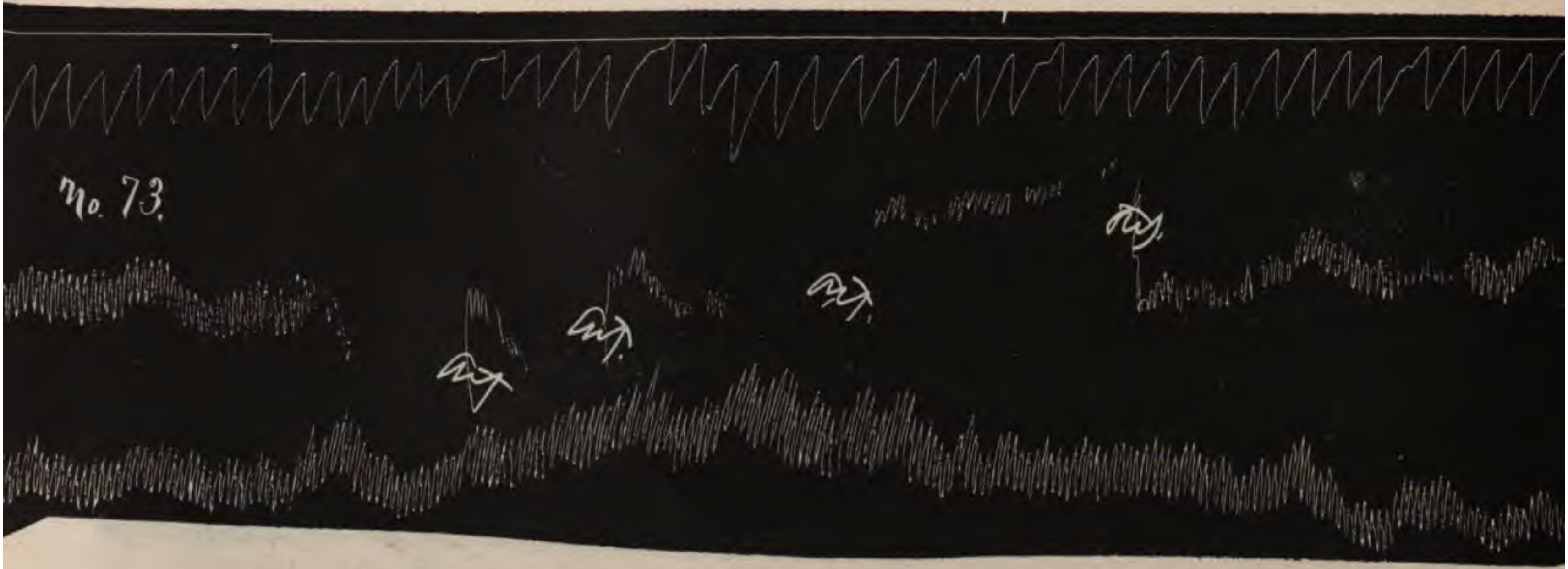

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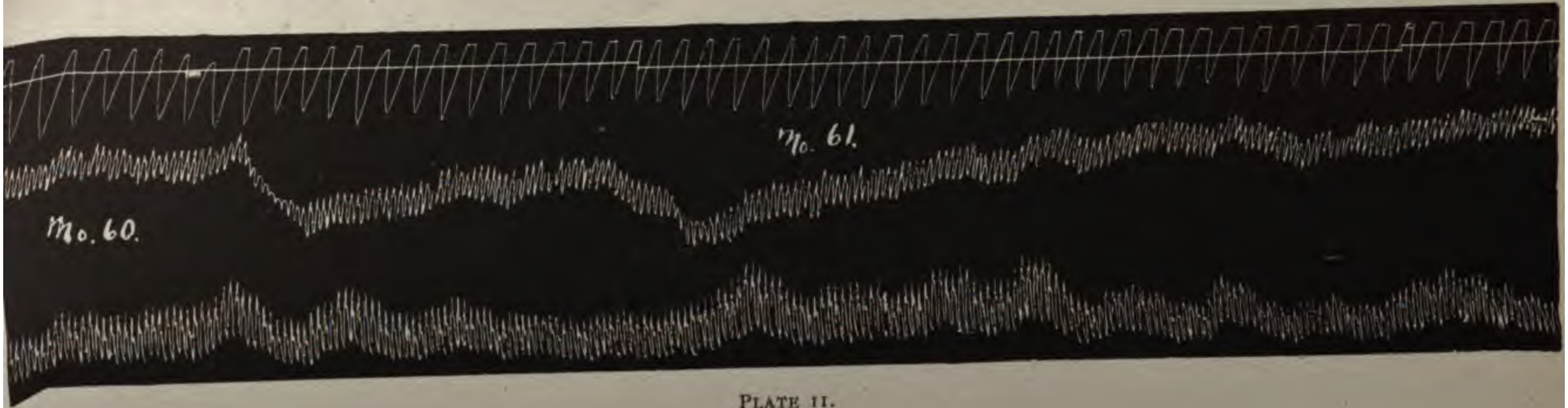

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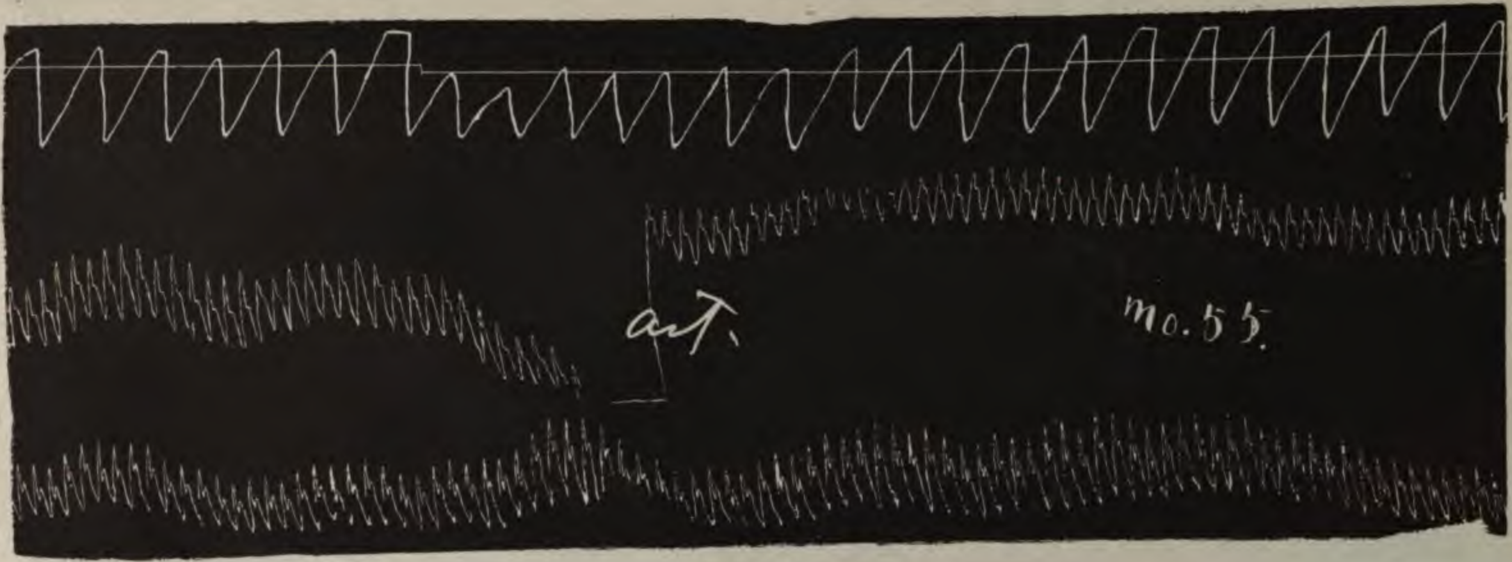

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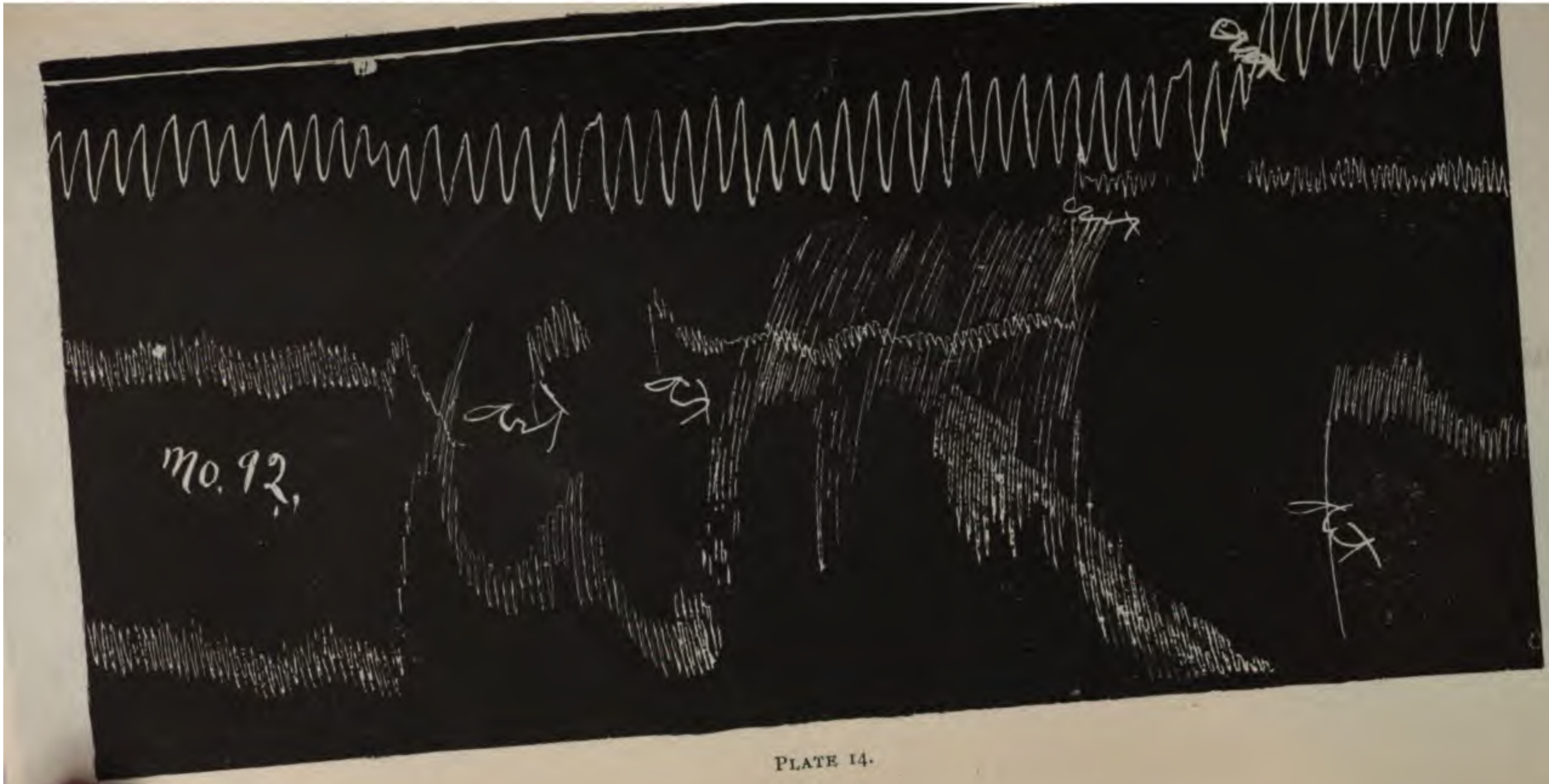

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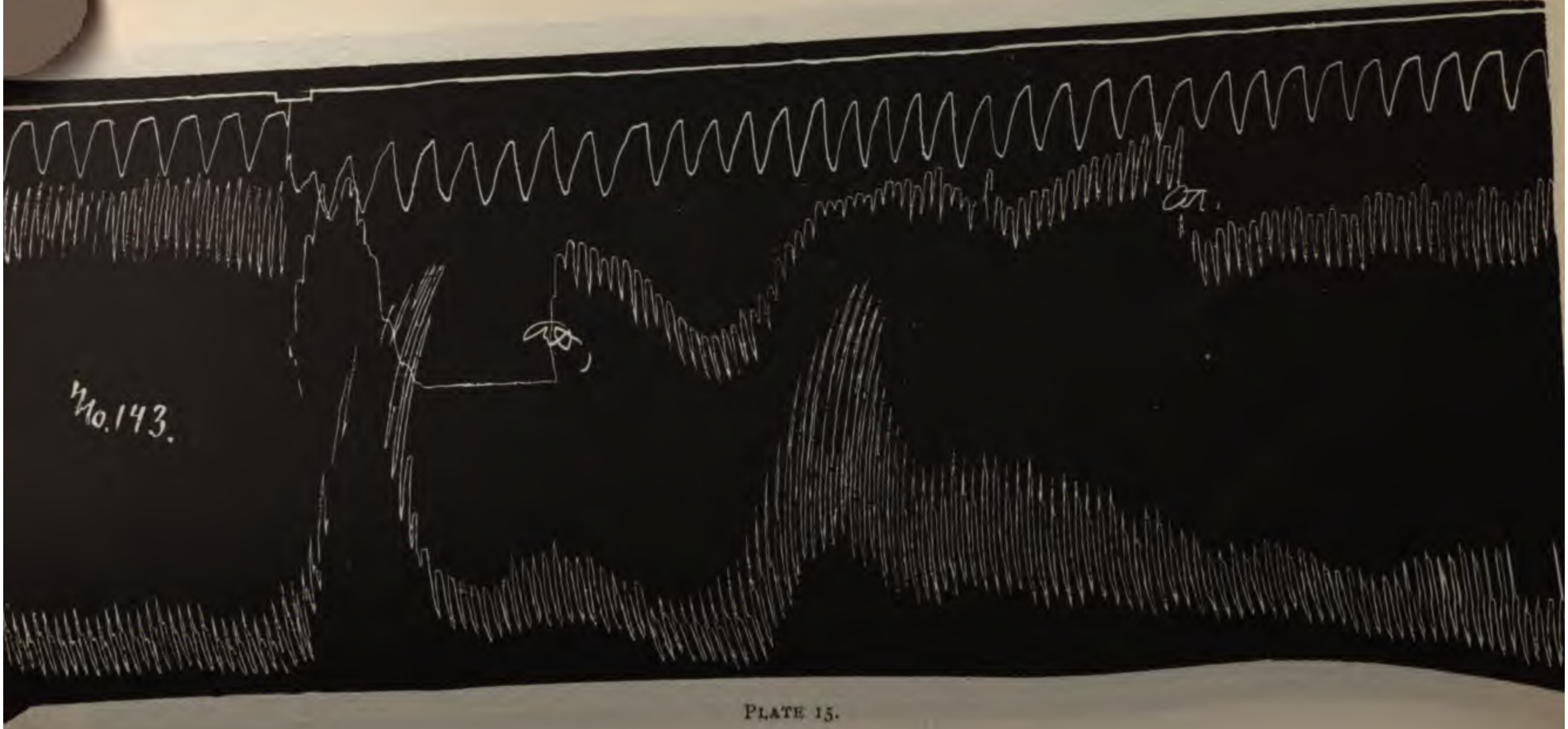

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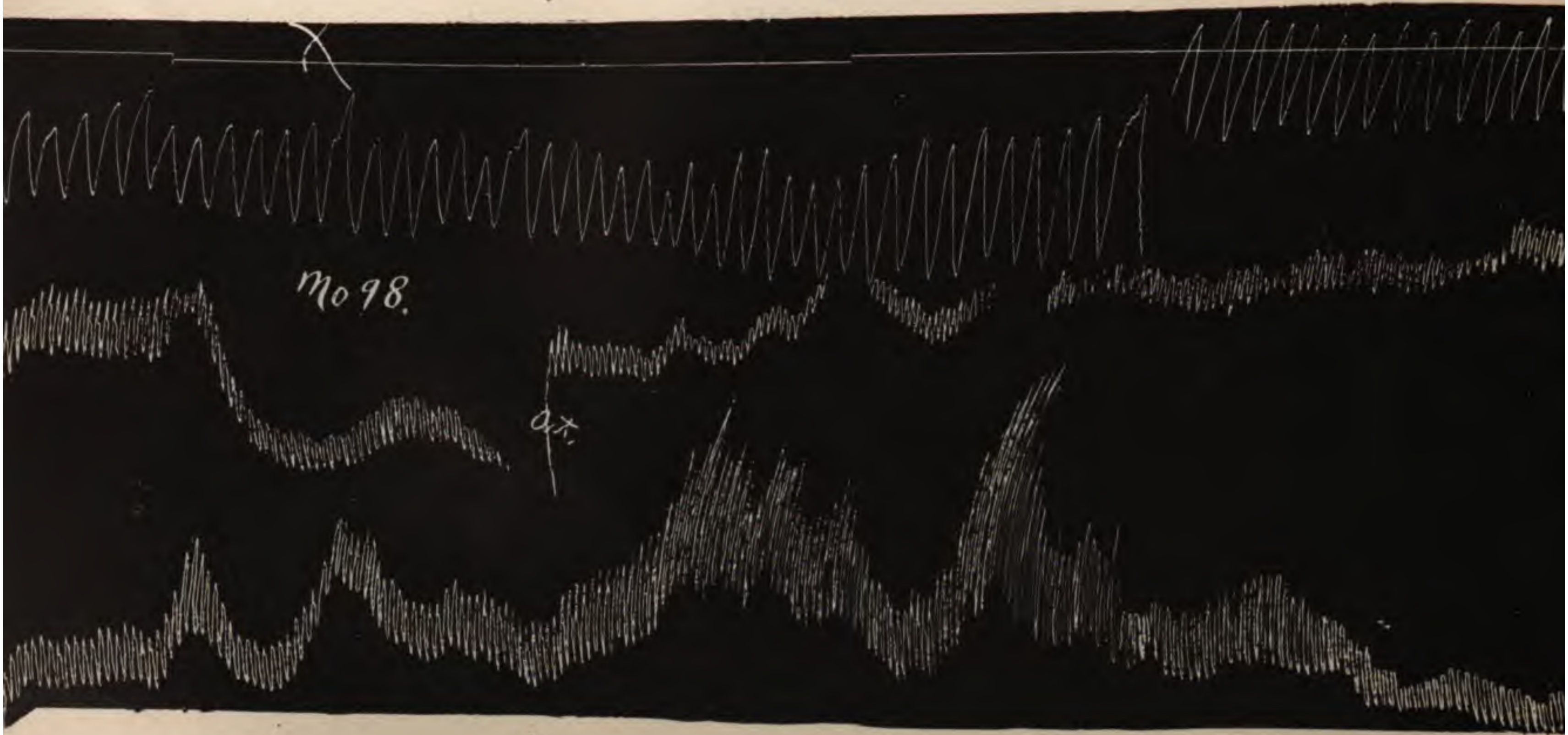

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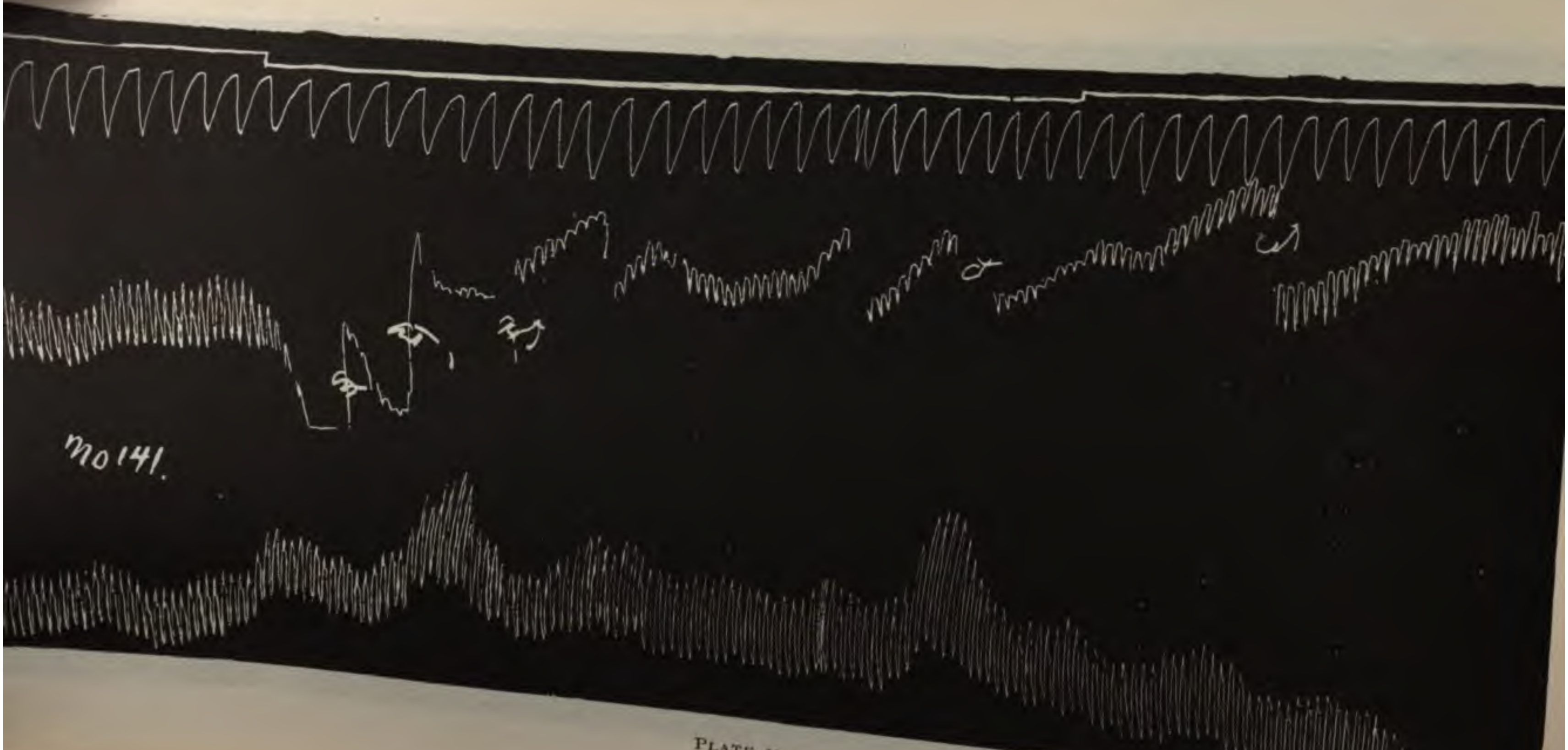

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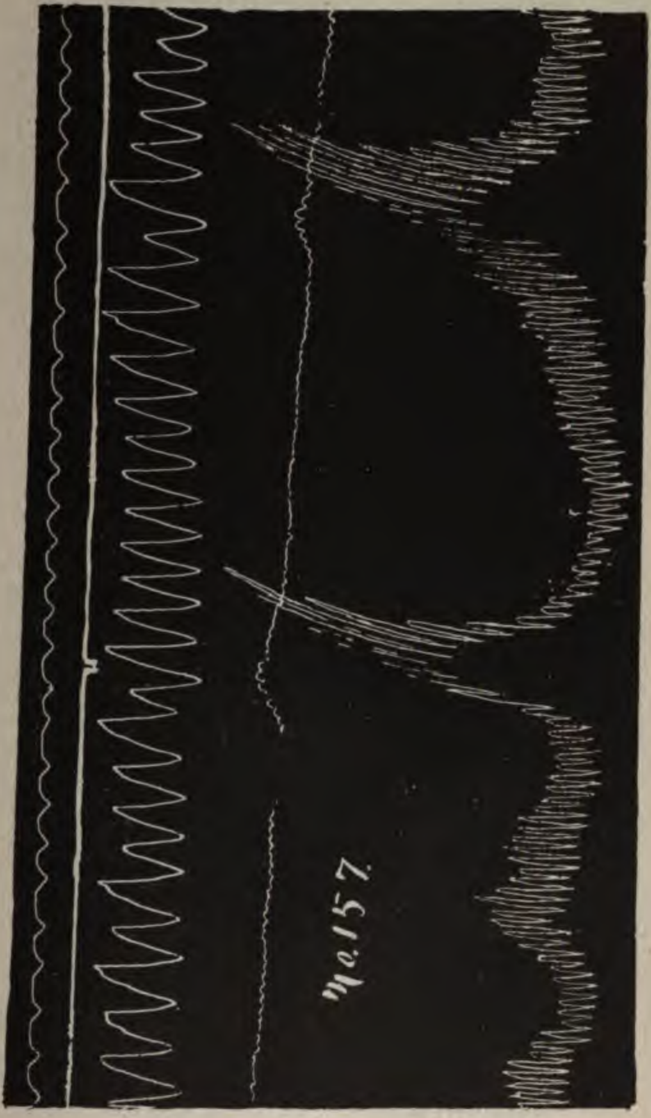

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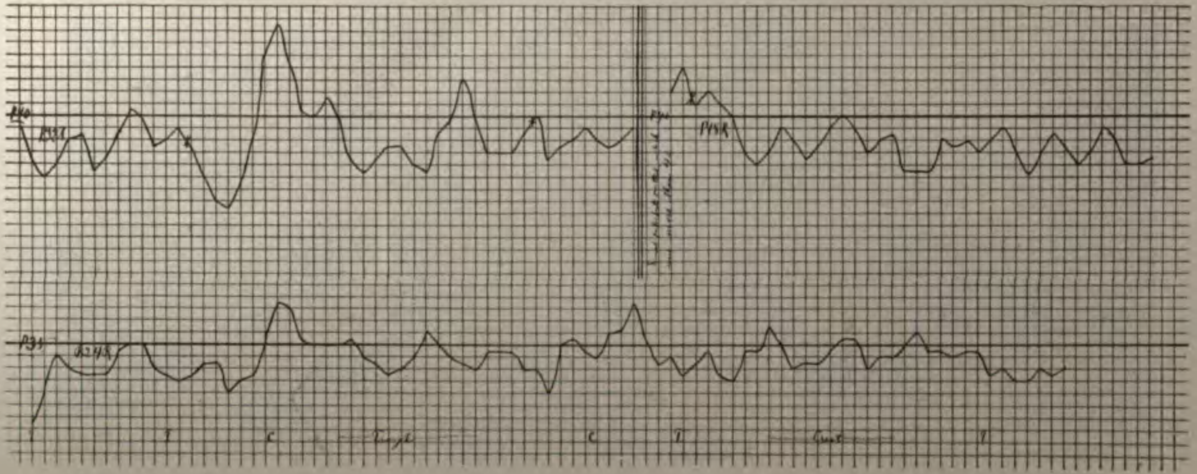

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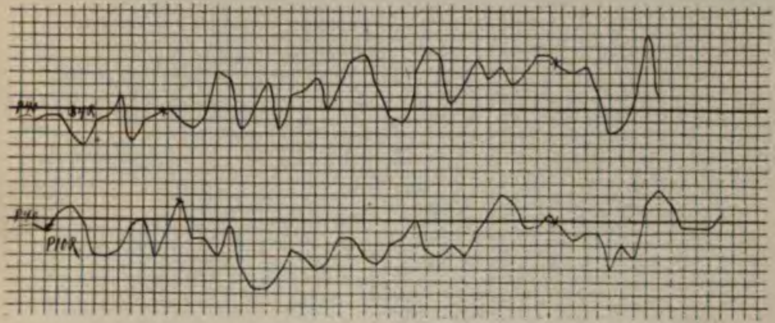

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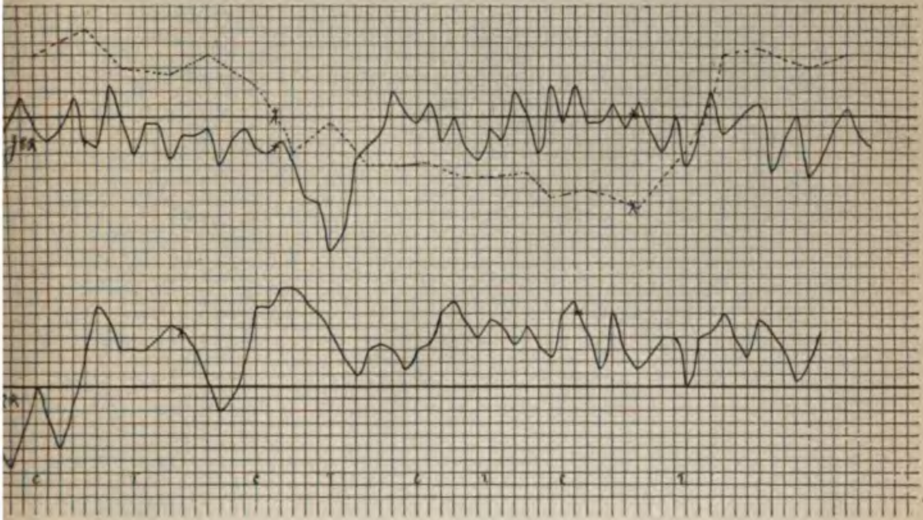

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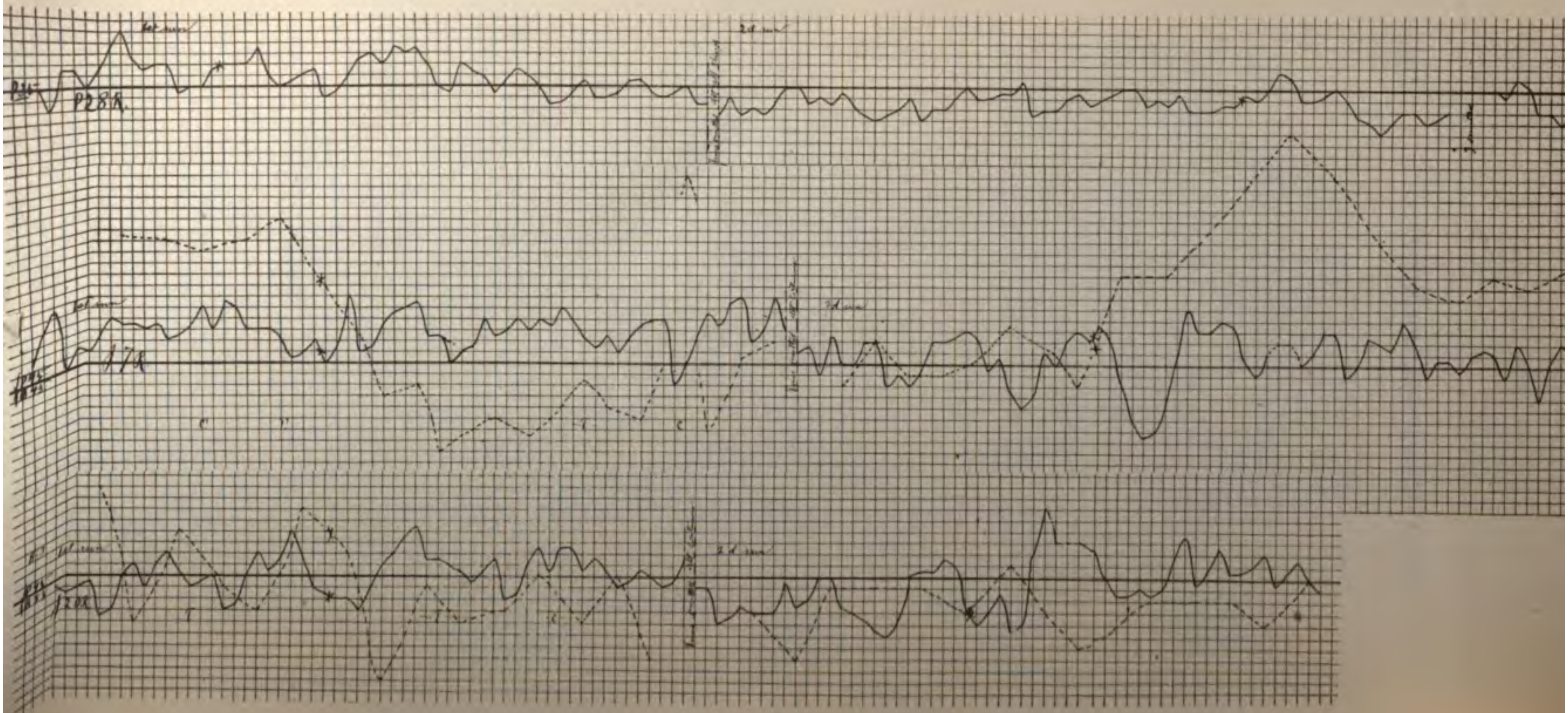
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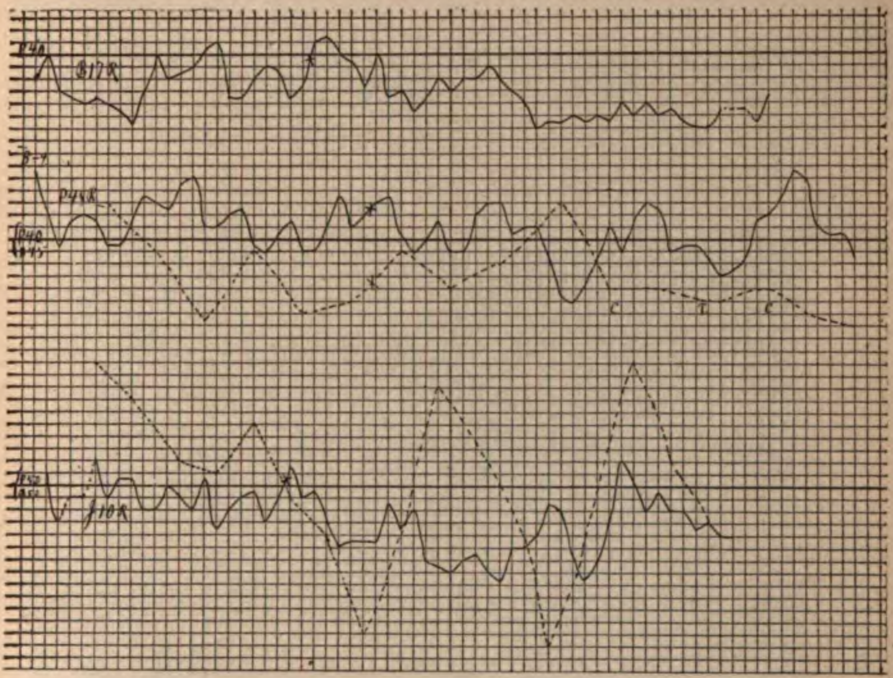
PLATE 22.



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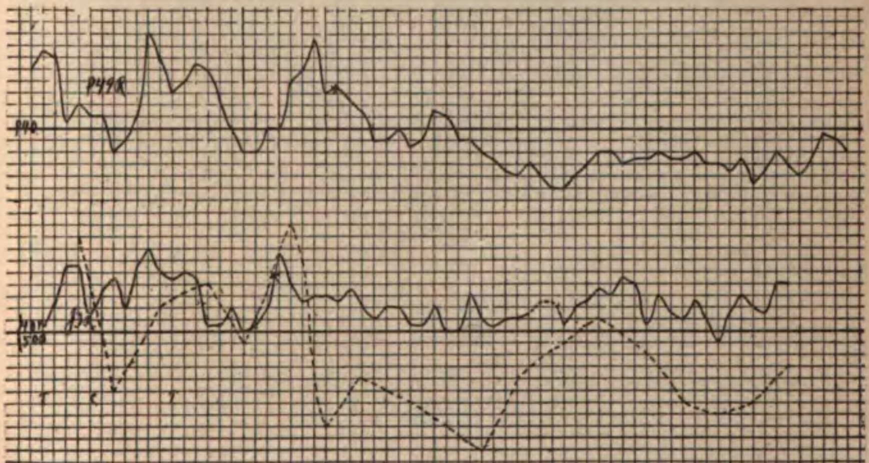

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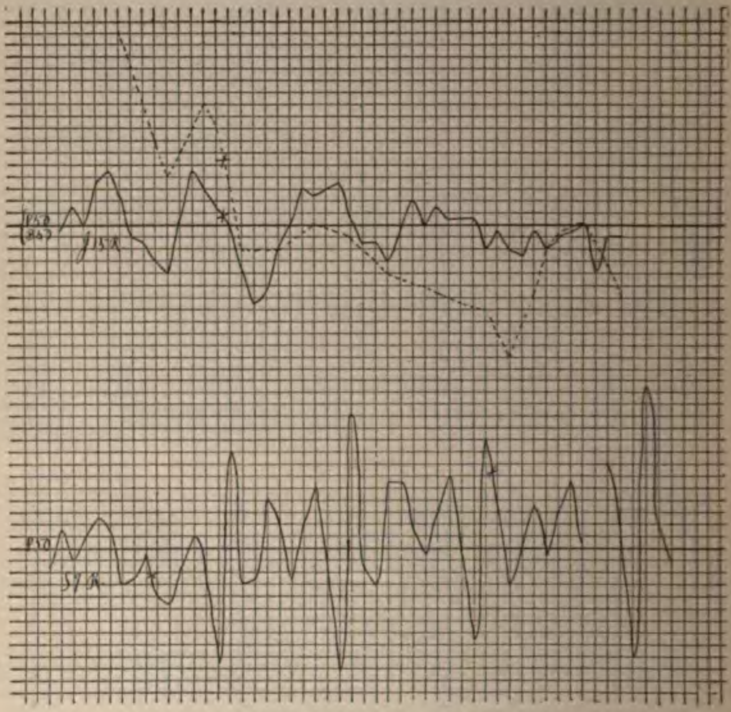

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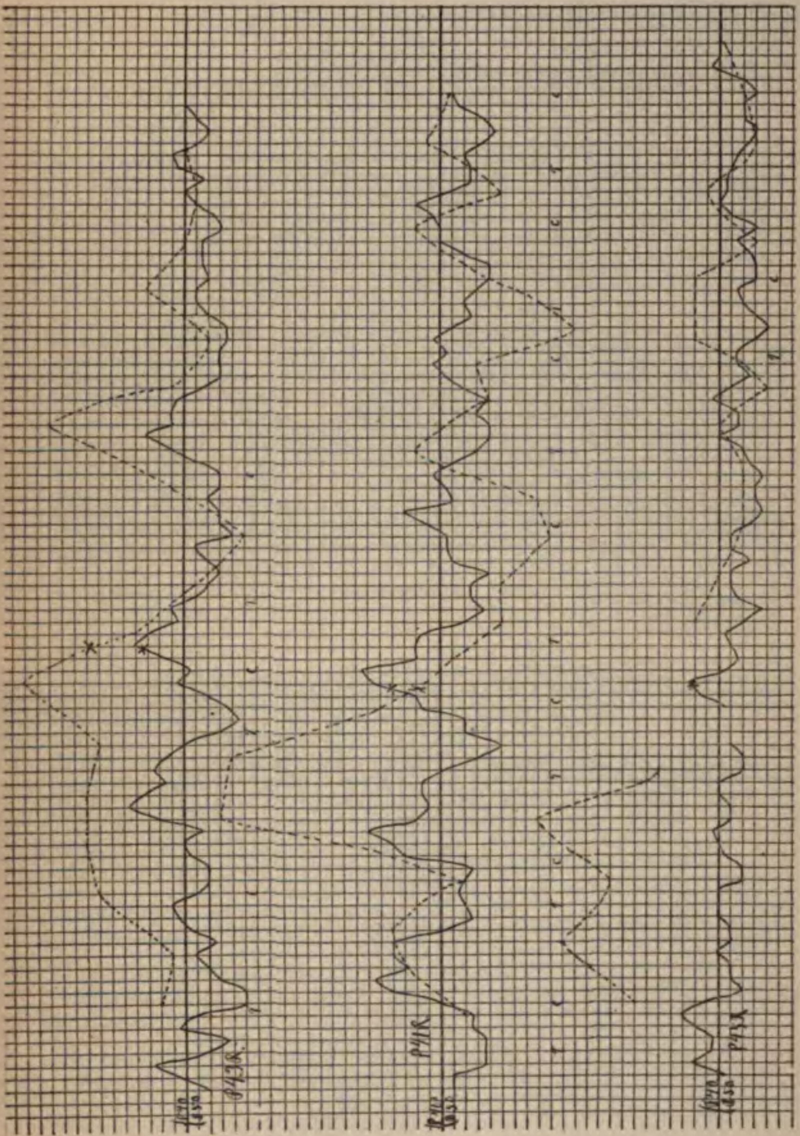

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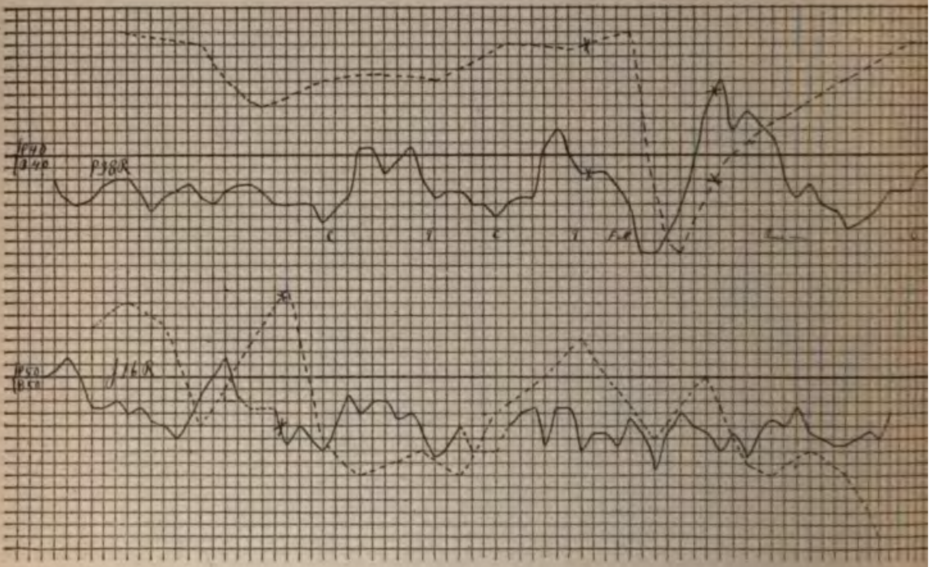

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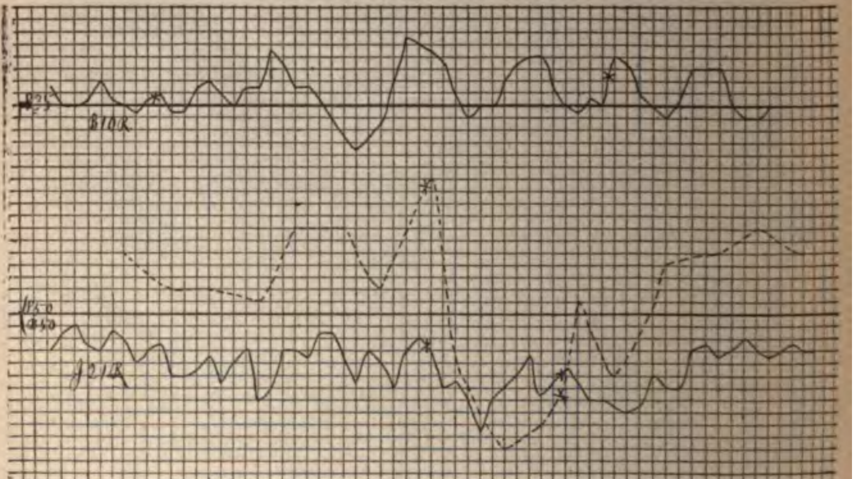

PLATE 28.

PSYCHOLOGICAL LITERATURE.

Experimental Psychology, A Manual of Laboratory Practice, by EDWARD BRADFORD TITCHENER. Volume II, Quantitative Experiments: Part I, Student's Manual, Part II, Instructor's Manual. The Macmillan Company, New York, 1905. Student's Manual, pp. xli + 208. Instructor's Manual, pp. clxxi + 453.

In the field of quantitative psychology English and American literature has been relatively sterile. It would be interesting to determine to what causes this sterility is attributable, but for the present at least, we must forego the inquiry. Meantime in passing it may be remarked that this lack of productivity has been referred by certain presumably competent observers to a deep-seated Anglo-Saxon suspicion of the utility of the results gained by psychopysical science under the Teutonic auspices where it originated. Others have asserted that psychophysics required a larger measure of accurate patience and a broader acquaintance with mathematics than the average psychologist of British or American lineage possessed. Still others of a more amiable mood have assigned as a cause the more urgent appeal made to English and American temperaments by the less technical aspects of psychology. It could hardly rank as a bold speculation were one to suggest a combination of these considerations, together perhaps, with others of like ilk, as responsible for the extant situation. In any case, whatever the reasons, the facts are plain. Psychophysics has been chiefly a continental product and until recently almost exclusively German at that.

In the volumes now before us, Professor Titchener, who completes herewith his series of laboratory manuals, has made the first systematic contribution to quantitative psychology which has appeared in the English language on a scale sufficiently large to do justice to the subject. If it enjoyed no other distinction, this circumstance would entitle the achievement to high regard. But its claims to professional and scholarly consideration are of a more impressive kind. It seems to the reviewer a safe prophecy that these volumes, with the revisions which they are likely to undergo, will form for a generation at least the classical English treatment of the subjects with which they deal. Nor is this judgment indicative of entire agreement with the author upon the positions adopted in the text. It rests upon the breadth and solidity of the work and upon the lucidity of the exposition. One might quite conceivably differ from Professor Titchener on every important point in his books and still cherish the sincerest respect for his accomplishment and the most sensitive appreciation of the service he has rendered English-reading students of psychophysics. Indeed, the reviewer is not disposed to limit his sense of the usefulness of the volumes to readers of this language. How far, however, our foreign colleagues will profit by them remains to be seen.

Before entering upon a more detailed examination of the books and the author's positions upon the great controverted issues of psychophysics, it seems fit to comment upon the monumental labor which has been dedicated to the task of composition. This is the more appropriate in view of the disposition prevailing in sundry quarters to toss off psychological volumes between meals. The literary and bibliographical completeness of these volumes, together with the ripe and sober estimates which they contain of complex problems, establish a most wholesome standard for English writing. In this particular they seem to the reviewer to mark a distinct advance upon their forerunners, the volumes on qualitative investigations. The typographical work has also been well done, although errors are by no means wanting.

The first question of general significance raised by these treatises concerns the soundness and expediency of the division of the experimental field into a qualitative and a quantitative portion. The reviewer commented upon this matter in a notice of the earlier volumes, but it merits a further word in the light of the volumes now before us which reveal in complete form the author's conception of the distinction.

As the reviewer understands him Professor Titchener maintains that a knowledge of the quantitative relations of mental processes is as much a fundamental part of psychology as the knowledge of their qualitative peculiarities. The manuals themselves furnish the best evidence that from the practical point of view the division is not only feasible but also pedagogically useful and scientifically warranted. It enables one to present the field of experimental psychology with due regard to those lines of cleavage which have been historically significant and it reflects a distinction in methodological procedure that fully justifies recognition in some form as radical as this embodied in the author's devotion of separate volumes to its exposition.

If the isolation from one another of the quantitative and the qualitative experiments has any practical drawbacks, it is more likely that these will be found to inhere in the conveyance of exaggerated impressions of the ultimacy of the division and the exhaustiveness of the two branches of psychological inquiry. The two provinces are integral portions of the general psychological realm, however disparate their methods and aims may be. Moreover, there are other methods and points of view, *e.g.*, the genetic, which are equally valid and thoroughly promising. Of course considerations of this kind have weight only in connection with the more or less unconscious influences exercised by a text over the minds of young students. No one would think of accusing Professor Titchener himself of lack of breadth in his conception of the legitimate psychological methods.

On the theoretical side, the division raises a question of scientific classification which is intrinsically interesting and of peculiar import in this immediate instance because the reply given by our author [as will presently appear] is in reality based upon the most fundamental prepossession in his treatise, *i. e.*, the nature of mental measurement.

In our general works on psychology there is often no substantial distinction made between psychophysics and quantitative psychology. Nor is this strange when one remembers that until recently most of the English writing on matters pertaining to this range of problems was done under the controling influence of Fechner for whom the dominant interests were psychophysical in the precise meaning of the term. From the point of view of our author, however, the distinction is at once real and pregnant. Psychophysics is but one chapter in a quantitative psychology. Strictly considered, the fundamental interest in psychophysics proper is always centered in the discovery of the quantitative relations between consciousness and the physical world, whether the latter is represented by processes in the animal body, or by the extra-organic molecular movements constituting a physiological stimulus. As distinguished from this conception of psychophysics quantitative psychology finds its problem in analyzing the quantitative aspects of mental processes *per se* and quite apart from any overt reference to the physical world. It must be admitted that in actual practice the difference between the two forms of inquiry often dwindles to the vanishing point. But this does not invalidate the fact that the ultimate aims of the two are distinct. Moreover, in many cases the procedure is different. But however tenable this line of demarcation, if accepted, it instantly raises the question of the status of psychophysics in psychology. Does psychophysics fall definitely inside the range of psychology?

Whether psychophysics is to be accepted frankly into the family as a genuine member of the psychological flock obviously depends altogether upon one's conception of the scope of psychology. It is quite possible to view psychology in such a way as to render psychophysics at best a hermaphrodite form with a dubious position. For example, if one considers the proper business of psychology to be the analysis of consciousness conceived in common-sense severance from the world of physics, it would seem to follow that psychophysics as an end in itself must fall somewhat outside the barriers, or at all events must sustain to the main stream of psychological interest a purely ancillary relation. If one, on the other hand, accords to the jurisdiction of psychology anything which comes to hand in the search after knowledge about the mind, it is entirely possible to welcome psychophysics as an orthodox member of the fold. The definitions of most contemporary psychologists probably look toward the first conception, but their practice quite as certainly looks in the other direction. At least the reviewer recalls no psychologist who does not gladly invade any territory contiguous to his own when the raid promises booty of a valuable kind. This is illustrated constantly and somewhat flagrantly by the familiar citations of materials gained from neurology which can hardly by any stretch of the imagination be regarded as properly psychological ground, when one limits this ground to the immediate analysis of consciousness as such.

On the most fundamental and significant problem of the quantitative psychology, *i. e.*, the possibility of, and the nature of, mental measurement, the author is frankly a disciple of Delbœuf. The material of mental measurement is not sensation as such but (in the range of sensory processes, of which alone we shall speak here) sense distances, contrasts or relations between sense excitations. This view at once forestalls a basal weakness in the original Fechnerian position against which armies of critics have directed their fire. It avoids the necessity for defending sensation as itself a measurable magnitude. Indeed, it goes further and maintains, as do certain mathematicians, that such an isolated quantity as would be afforded by a sensation possessing magnitude of and by itself is absurd and impossible. Meantime it vigorously champions the measurable and quantitative nature of the sense distances.

The other half of the fundamental perplexity confronting a quantitative psychology [for the first point to which we have just referred is but a part of a larger whole], concerns the determination of a unit of measurement. This also is solved on Delbœufean lines by resort to increments of sense distances which prove in actual practice to be highly similar to the Fechnerian increments of sensation, although the conception of them is different in theory and their place in a psychophysics is different from that accorded by Fechner to his increments. Sensation is not itself regarded as constituted by integrated increments, but the sense distances are measurable in terms of noticeable incremental differences. The unit is arbitrary but justifiable because practicable.

Evidently the utility of such a unit will depend upon its equality over the whole range of sense excitation to which it may be desirable to apply measurement. This fact calls to mind the old familiar contest which Fechner carried on with his adversaries. He stoutly maintained that the increments were equal in different parts of the sense scale. Many of his critics have insisted that this contention flies directly in the face of the most obvious introspection, and to the rank and file of non-psychophysical psychologists their position has generally seemed valid. They urge as an insuperable difficulty the alleged fact that in the field of pressure sensations, for example, the just noticeable difference which enables us to discriminate a gram from some other heavier weight does not seem to us at all the same as the j. n. d., which permits us to distinguish between a weight of a hundred pounds and another proportionately heavier. On this issue Professor Titchener is a rather guarded upholder of the equality of the increments of sense distance throughout the intensive scale. His attitude is determined in part (so far as concerns the supraliminal distances) by his own introspective deliverances, and in part by the fact that the results of measurements made on the assumption that the increments are practically equal apparently substantiate the claim. At all events they do not militate against it. The reviewer cannot refrain from remarking that with regard to difficulties of the kind here involved a functional psychology occupies a peculiarly strategic position. A structural psychology may well be in doubt about the equality of the these j. n. d. increments. Functionally they are unquestionably equal and this would appear to be the point of really fundamental moment.

It has probably been evident that everything which has been said up to this point has direct and immediate bearing on the intensity relations of psychic processes. But the circumstances which attach to the quantitative relations of a spatial and temporal kind and to the serial arrangements of a qualitative sort are not sufficiently different in nature to necessitate a separate treatment at this juncture.

A large part of the discussion about mental measurement has been pertinent primarily to the theoretical rather than to the practical interests involved. Nor is the cogency of the outcome particularly flattering to the lucidity of the protagonists. Indeed, much of the controversial writing suggests souls calling to one another across oceans of misunderstanding, and unhappily the calls are not always agreeably toned at that. Professor Titchener's citations exhibit strikingly the extent to which utterly divergent and ultra-respectable scientific opinion may be cherished on the theoretical questions at stake. By something of a *tour de force* he manages to discover strains of agreement in a number of the important authorities, but the achievement has impressed the reviewer as the worthy effort of a peacemaker and a searcher for harmony, rather than as the disinterested registration of an obvious fact.

But whether one adopts the general views of Delbœuf, or of Wundt or Müller, or even of Fechner, there are many points in one's practical procedure which might remain substantially unaltered. For example, one might employ the method of minimal change without essential modification, whether one were primarily engaged in determining a limen in the sense of Fechner, or a serial determination of intensive sense distances in Delbœuf's sense. Of all of which the point is that one must distinguish sharply between questions of technique in procedure and questions of theory as regards the status of quantified method in psychology. The two issues are of course vitally related from the standpoint of any ultimate view, but an agreement on practical procedure is as compatible with divergent beliefs about the theoretical matters involved, as is agreement about physical measuring with discrepant notions of the nature of matter. On the question of practical procedure, there is already extensive agreement and doubtless will speedily be more as the real basis of the differences of opinion becomes clarified. On the underlying problems of a theoretical character the decadence of controversy will probably be much slower, but fortunately it is practically much less important.

It would be pretentious to attempt an exhaustive critique of Delbœuf's view of the subject of mental measurement, but a mere word of commentary may be permitted. Passing over such refinements of criticism as Meinong's (not as valueless, but as momentarily irrelevant), and disregarding the advocates of mental mensuration as primarily applicable to processes of judgment and apperception (for whose views there is much to be said), it appears that the position of Delbœuf, while superior to its predecessors as a practical working foundation. has not altogether escaped certain of the limitations which hedged them about. Two of these may be mentioned.

In the first place, however strenuously it may be urged that these Delbœufean measurements are mental and not physical measurements, it still remains true that all expressions of them, all tangible results of them must be formulated with the assistance of physical terms. This is perhaps nothing to their discredit as compared with physical measurements, which require not only physical units, but also an observing mind to make the measurements. The two are at quits on this score. But the facts warrant notice whenever emphasis is being put upon the *purely* mental character of the Delbœufean measurements. Physical stimuli are constantly in evidence whenever specific quantitative terms are desired. Nor do we forget in making this assertion that these stimuli are employed as means and never as ends in themselves. But pure mental quantification is apparently an invertebrate form which requires the stiffening of some kind of physical spine in order to render it practically available.

In the second place, the sense distance which is the measurable magnitude under this theory, psychologically considered, is something of an artifact, as were the Fechnerian increments of sensation. This does not prevent its successful employment for the purpose in hand (nor were the increments of Fechner intrinsically impractical), but it does in a degree affect its status as a psychological term. To illustrate, when one light is sensed as brighter than another just preceding, it does not seem to correspond to anything commonly experienced when the second light is apprehended, to say that the distance of the one from the other on an intensive scale is thereby established; nor is the situation altered by any such considerations as those set forth in Professor James' description (closely resembling in its immanent psychology certain of Fechner's conceptions) of the second sensation as being a sensation of light-B-brighter-than-light-A. It is felt as brighter without doubt, but in this awareness of superior brightness there is no necessary consciousness of the distance between the two as sense distance. That is to say, the immediate conscious reaction is "brighter," but the quantitative ranging of the experiences on an intensive scale, which is in some form or other essential to measurement, is a sophisticated afterthought of the professional psychologist. To read it in as a regular feature of intensive comparisons is to commit the psychologist's fallacy in a peculiarly obvious way.

To this line of attack Professor Titchener has already prepared a reply which relies for its force upon the contention that we often make quantitative judgments without being conscious of the quantity as quantity. The reviewer does not challenge this assertion but he raises the previous question and remarks that whereas the Fechnerian conception of mental measurement has frequently been characterized as in reality physical measurement, the Delbœufean conception in its turn offers a somewhat vicarious type of measurement, inasmuch as the serial arrangement indispensable to the real measuring process is often not overtly present at all to the consciousness upon which the measurements are being executed. These comments are offered in no spirit of hostile criticism, for the reviewer's attitude is distinctly sympathetic towards the Delbœufean view, but simply to emphasize the fact that there are residual difficulties, or at least limitations, for this theory as well as for its forerunners.

On one rather important matter closely related to the preceding topic, Professor Titchener seems to have fallen into a curious inconsistency. He says (Student's Manual, p. vi) that the question asked of consciousness in quantitative experiments is never the direct question 'how much,' but *always* one or other of the two questions 'present or absent' or 'same or different.' But on page 56 of the same book in describing the procedure for a test on the DL for brightness he speaks of the O saying at a certain point 'lighter.' And again on page 106 in giving directions for tests on the DL for pressure he says the "judgments may take the form 'much greater,' 'greater,' " etc. Now the reviewer has no desire to invest time in assaulting a man of straw and the author probably has some ready explanation for this seeming contradiction in his utterances. But whatever the facts as regards this part of the question, it appears to be reasonably evident that only on the basis of the ability of consciousness to give judgments of the "greater" or "less" type does it become possible to speak of conscious processes as being properly subject to arrangement in a quantitative series. It is perhaps possible so to devise one's experiments that the verbal reaction shall always be either "present or absent" or "same or different." But the actual conscious reaction either involves directly the quantitative predicate, implicitly if not on every occasion overtly, or consciousness is not susceptible to genuinely quantitative treatment. Even if one attempts so to arrange one's procedure that the question "how much" is never directly asked, there are certain experiments, *e.g.*, those on equality of spatial distances, in which this query insists on impinging upon the consciousness of the O, however scrupulously the E has endeavored to avoid it.

Before examining the detailed arrangement of his work we may note that Professor Titchener does all he can to aid in the worthy effort to put Weber's law in the subordinate position which it really deserves to occupy. German psychologists have for a long time ascribed to it a position of secondary importance, but in the writings of English and American psychologists it has persisted in monopolizing the foreground as constituting substantially the alpha and omega of accomplishment in quantitative psychology. To be sure this attitude is often varied by recognition that its demonstrated validity is limited to certain ranges of sense experience and valid therein only on terms of sufferance for irregularities. But even so it looms large. To the reviewer this fact seems attributable without serious doubt to the philosophical and cosmological interpretations accorded it. In any event we appear to be entering upon a period of larger enlightenment concerning the merits of the case.

Turning to the organization of the books we find that the volumes fall into three principal chapters of which the first is entitled "Preliminary Experiments" and consists of illustrative determinations of qualitative and intensive limens. The second is devoted to the metric methods and contains experiments illustrative of all the more important forms of quantitative procedure in the realm of sensation, and the third is given over to reaction experiments from the quantitative point of view, including a rather extensive expository account of electrical units and methods. A final and much briefer chapter discusses the psychology of time from the same standpoint. In addition to these divisions, the Instructor's Manual has an elaborate and invaluable introductory account of the rise and progress of quantitative psychology, which constitutes essentially a critical history of psychophysics, and this finds a much briefer counterpart in an introductory section in the Student's Manual describing the field of measurement in general and psychological measurements in particular. The Instructor's Manual has also a chapter on typical experiments in quantitative psychology and three appendices dedicated to examinations, books, instruments and dealers in them. Indices of various kinds complete the useful machinery with which the volumes are provided.

The amount of space reserved for discussion and explanation of electrical apparatus and electrical principles will no doubt appear to certain instructors excessive, while others will feel that the space devoted to purely mathematical exposition is unnecessarily extended. But the author has the comfort of knowing that no course which could be selected would please every one on this score. Meantime in the reviewer's opinion the plan pursued is altogether judicious and profitable. The unfathomed ignorance of the average student concerning the facts in the case passes belief unless one has for some years given instruction to classes in laboratory psychology.

So far as the reviewer has any quarrel with the portions of the text which deal with apparatus his criticism would be directed at the failure to characterize certain pieces as frankly as his own experience would warrant. The difficulties are no doubt often the fault of the mechanic and not defects in the theory of construction. But the results for the unsophisticated purchaser are one and the same. Thus, for instance, the Zeitsinn apparatus pictured on page 399 of the I. M. is by no means always so constructed as to permit satisfactory results. Nor does the author's reason for introducing Scripture's touch weights (S. M. p. 15.) seem convincing. He admits in a comment in the I. M. that the apparatus is unsatisfactory. The reviewer has encountered many sets of these weights and while he is certain that occasional sets must have been so constructed as to give reliable results, he is equally certain that the average set converts the experiment into a farce. So, too, the cartridge weights mentioned for experiments on pressure on page 189 of the I. M. have the advantage of cheapness and the endorsement of wide use, but they almost invariably wobble if used for any of the higher weights, *e. g.*, 100 grams, and they commonly require a pad to minimize temperature effects. This pad seldom transmits the pressure equally over the various parts of its surface. The same kind of limitation holds true of other pieces of listed material. Criticism of this sort would be sheer carping were not the author's dictum on these subjects so often taken uncritically as gospel. In consequence, a higher degree of responsibility rests on him than falls to the lot of most of us.

On one matter of practical importance to students *i. e.* the subjective criterion of assurance, the writer could wish that the author had given more specific instructions and advice. The difference between doubt and assurance is nowhere more strikingly brought out and nowhere in the realm of psychological studies more significant than in the case of quantitative experiments. Shall one, for example, establish as his standard for judging that two sensations are intensively different a conviction altogether unalterable, or shall one employ an attitude of practical assurance similar to that with which one accepts as true the recollection of one's whereabouts a week ago, or, finally, shall one cultivate a more highly sensitive appreciation and give a positive judgment whenever one suspects a balance in favor of a difference? Any one of these attitudes may give relatively consistent results if adopted and conscientiously carried through. But there is of course a choice and there may be other attitudes still more judicious.

In the writer's experience the personal equation comes in here in a very disturbing manner and students are nonplussed and often discouraged, if not disgusted, to find that several of them have been proceeding in diverse ways when supposedly using similar methods. Moreover, the difficulty is likely to obtrude itself on the student's attention despite every precaution on the part of the instructor. Of course every teacher gives his own students directions upon this familiar difficulty of quantitative work, but a fuller discussion than has been offered by Professor Titchener would have been most welcome and of the greatest value in its tendency toward uniformity of understanding if not of procedure. Apart from the purely psychological issue involved, the point really raises the whole question of the nature of certain of the measurements and the part played by errors of observation.

On the important matters relating to the specific psychophysic methods the author has obviously been much influenced by Professor G. E. Müller. These questions are taken up in the second main part of the texts. In the S. M. attention is chiefly confined to directions for the application of the technique. The I. M., as was noticed above, offers a most valuable historical and critical exegesis. The methods are discussed under the following headings: (a) the method of limits in its various forms (at present commonly designated in English as the method of minimal change); (b) the method of average error with its subordinate form the method of equivalents; (c) the method of equal sense distances generally known in connection with its several modes as the 'gradation methods;' and (d) the method of constant stimulus and constant stimulus differences, historically familiar as the method of right and wrong cases.

To readers accustomed to the older nomenclature the terms used to designate the different methods seem somewhat strange and possibly a trifle cumbrous. But on the whole (with one possible exception, *i. e.* certain of the forms of minimal change) they are intrinsically distinct improvements upon those commonly in vogue, for they suggest more immediately and less ambiguously than do these the essential characteristics of each.

As is well known, the proposed classification of the methods are almost as numerous as the psychophysical investigators. From the student's point of view, the main consideration is that the classification offered shall display the relations most essential for both theory and practice in an intelligible manner, and this result is accomplished successfully by the arrangement adopted by our author. It possibly tends, however, to emphasize differences at the cost of obscuring similarities, and the reviewer is strongly of the opinion that a more overt and explicit discussion than occurs anywhere in the text justifying the usage employed would have been a great boon to students and a material addition to the value of the treatises. The author's labors peculiarly fit him to handle with lucidity and relative ease the complex problems and relations involved.

As compared with Professor Titchener's previous books, the present volumes suggest a higher nervous tension in composition. He has evidently welcomed the opportunity to free his mind on a number of subjects in which his emotions are somewhat concerned. The criticisms are often sharp to the point of the caustic and more personally toned than heretofore. This tendency, suggestive of the fruits of long contact with German usages and particularly with Professor G. E. Müller, that acrimonious guardian of the psychological fold, the reviewer regards with chastened enthusiasm. To be sure the technical tedium of the text is considerably relieved by these excursus, but the books were not written primarily for entertainment. For the most part, be it said, the criticism is perfectly objective, as scientific criticism seemingly should be. Moreover, the author sins as little in this way as any psychologist known to the writer.

In conclusion the reviewer is moved to express a measure of dissent from Professor Titchener's estimate of the importance of the spread of interest and proficiency in quantitative psychology. The difference is perhaps largely one of degree of emphasis. To the writer it does not seem probable that the extended development of quantitative investigations is an immediate *sine qua non* of further progress in psychology. So far as these methods invite and stimulate exactness and accuracy and appreciation of scholarly modes of work, so far will a wide-spread familiarity with them in psychological circles be rewarded by an increase in the amount and scientific character of the output of our investigators. The reviewer has always felt the most unreserved confidence in the improvement which would accrue to the qualitative studies from a more thorough acquaintance with the quantitative procedures. As every experimentalist knows, there is hardly a problem in qualitative psychology in which some trace of the quantitative element is not to be found, and in practically all experiments, a regard for the canons of such procedure is indispensable to trustworthy results. For their general tonic effects, therefore, as well as for their practical utility in qualitative research, the reviewer looks with great hopefulness upon the development of interest in quantitative methods and problems. Nor would the reviewer be understood as depreciating the value of quantitative work on its own merits and for its intrinsic worth. Quite the contrary. But as the writer understands him, Professor Titchener would take a more extreme and positive attitude, with the conviction that we have gone about as far as we safely can without assistance from the quantitative side. The reviewer feels that there is yet much useful pioneer work to be done before the nicety of the quantitative methods can be summoned to trim up the edges. All this is matter of opinion pure and simple and time alone will determine the more correct estimate. For better or for worse there can be no question that these volumes will instigate a large amount of intelligent interest where before there was nothing but ignorance and hearsay. And no doubt, too, out of this new-born interest will blossom much research of a high order. For all this and for much more Professor Titchener will have our gratitude and appreciation. JAMES ROWLAND ANGELL.

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L'Attention, by W. B. PILLSBURY. Bibliotheque internationale de psychologie expérimentale. Octave Doin, Paris, 1906. pp. 299.

Professor Pillsbury and his pupils at the University of Michigan, have for some time been devoting experimental study to the so-called fluctuations of attention. The present book, however, refers only in passing to this work; it is a general treatise undertaking to summarize the facts about attention and to include them under a theory. The facts are grouped in the first eight chapters, entitled 'The

Psychic Effects of Attention;' 'The Motor Phenomena Accompanying Attention;' 'The Conditions of Attention;' 'Interest and the Feeling of Activity;' 'The Effects of Attention on Consciousness;' 'Attention and Ideas;' 'Attention and Association in Perception;' 'Attention in Memory.' The essence of attention as a mental phenomenon is declared to be increased clearness, and increased intensity is held under certain conditions also to occur. The motor phenomena of attention often follow attention proper, hence do not constitute it. To explain the fluctuations of attention, fatigue of the cortical cells, both those involved in the sensations attended to and those corresponding to associated ideas (the Lange memory-image theory) is invoked, but the length of the attention wave is supposed to be determined by vaso-motor and respiratory rhythms. The latter by themselves would not explain cases of rivalry, where processes in two sets of cortical centres are alternately dominant. The former alone would not explain the rhythmic regularity of the changes. Dr. Pillsbury's idea, as explained on p. 101, apparently is that in rivalry, at the moment of the general depression produced by the trough of a vaso-motor wave the unfatigued set of cells has an advantage over the fatigued set and a chance to assume dominance.

Under the head of 'Conditions of Attention,' the writer emphasizes the distinction between objective and subjective conditions. The former comprise the intensity, extent and duration of the stimulus; the latter, the contents of consciousness at the moment of attention, and farther back, the education, social surroundings and heredity of the individual. The distinction is not clearly drawn, however; for instance, movement is placed in the subjective class because its efficiency in directing attention depends on the biological conditions of mental development, while change in the intensity of a stimulus is referred to the objective class. Surely both are subjective if dependence on evolutionary conditions is the test of subjectivity, and both objective if that term designates characteristics belonging to the stimulus itself. One of the conclusions of the chapter on 'Attention and Ideas,' is that "every centrally excited process . . . is the result of two series of conditions, objective-associational and subjective-attentional." Thus the suggestion is clearly made that the essential conditions of attention are the subjective ones. We shall return to this point later.

Professor Pillsbury's discussion of theories of attention is clear and valuable. He divides them into three groups: those which relate attention to apperception, those which like the motor theory of Ribot elevate some accompanying phenomenon to the rank of an essential feature, and those which are purely physiological in their character. In discussing Wundt's apperception theory, the author makes the statement that Münsterberg has in the last ten years completely changed his position: while he formerly objected to explaining anything in consciousness by an unknown element outside of consciousness, "he now maintains that all consciousness is simply the manifestation of an unknown will." This position Dr. Pillsbury declares to be the counterpart of Wundt's apperception doctrine (p. 182). Münsterberg is clearly misrepresented here; he does not use his 'unknown will' to explain anything in consciousness. It remains forever removed from the world of science where explanations hold sway, and the only possibility of explaining conscious processes lies for him on the physiological side. It is perhaps connected with this misapprehension that Münsterberg's central physiological theory is nowhere mentioned in the book.

The chapter on 'The Physiology of Attention' contains the ablest and most original part of Dr. Pillsbury's work. In the preceding chapter he accepts the evidence put forth in behalf of the frontal lobes as the anatomical substrate of attention. He now points out that various experimental data, for instance, the results of Bowditch, Lombard and Warren on the knee-jerk, indicate that one nerve centre may affect another by way of inhibition and reinforcement. The work of Bruce on retinal rivalry and that of Taylor on the fluctuations of the Masson gray ring as affected by additional stimuli, may be interpreted as showing that such inhibition and reinforcement operate in the field of attention. The experiments of Lange on the effect of associated ideas in holding before the attention one interpretation of an ambiguous figure such as the outline cube, indicate that the centres chiefly active in such reinforcement and inhibition are those especially associated with the centres affected. Dr. Pillsbury prefers to allow the possibility of both reinforcement and inhibition, rather than to depend solely upon inhibition, after the Wundtian fashion, because he thinks there is evidence for positive effects of attention, such as increase in the intensity of a sensation. Attention, then, is explained physiologically as the result of the reinforcing action of certain cortical cells upon other associated cortical cells, plus a probable inhibitory effect of the former upon the rest of the cortex. What distinguishes it from association? This primarily: that it is an effect of one group of cells upon another, produced through the medium of the frontal lobes, while association may take place, as Flechsig supposes, directly between the various sensory regions of the cortex. The objective factors in association depend probably upon such direct connection; the subjective factors, involving the individual's whole past and heredity, must depend upon an organization of the entire cortex through the frontal lobes. There are three principal differences between that action of one group of cells upon another which is involved in attention and that which is involved in association. First, in the former case, such action is not the sole or principal factor exciting the affected group, as it is in the latter case; second, in attention the process in the cells which exercise the modifying influence need not be accompanied by consciousness, while it ordinarily is so accompanied in association; third, the effect in attention is produced by a general participation of the whole cortex, while in association only a localized group of cells is active.

Space forbids a discussion of the interesting applications which Professor Pillsbury makes of his theory to pathological phenomena. It is perhaps most valuable in its definition of the relations between attention and association. The working out of the psychological aspect of the author's theory in the earlier chapters seems, however, less clearly conceived than that of its physiological basis. If objective relations between conscious processes depend physiologically upon mere associative connections between sensory centres, while only the subjective factors involve the reaction of the whole cortex through the frontal lobes, why should we have objective and subjective conditions of attention distinguished, and imperfectly distinguished at that, as we have seen in the earlier chapters? As a matter of fact there are no purely objective conditions of attention. Mere intensity, for example, does not condition attention. When I ride in a trolley through a crowded city street, my eyes are dazzled and my ears deafened by intense stimuli which may pass almost wholly unnoticed as I occupy myself with a train of thought. Sudden increase in intensity does attract attention, because it appeals to an instinct based far back in the part of the species, and is therefore classed among the subjective conditions of attention. We attend to nothing save through the reaction of the whole cortex, hence we attend to nothing for really objective reasons.

A word or two is necessary in regard to the translation. It is unfortunate that we should not have had this scholarly and suggestive book in its native English, for the French version is not always clear and is occasionally incorrect. To mention only two examples of the latter fact, when Dr. Pillsbury speaks of 'the mood of the moment' as among the subjective conditions of attention, 'le mode du moment' is meaningless as a translation. When he refers to "Stout's 'tendency towards an end,'" meaning the principle thus designated by Stout, there is an ominous suggestion in the translators' 'la tendance de Stout vers une fin'!

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PSYCHIATRICAL LITERATURE.

By ISADOR H. CORIAT, M. D.

Les Écrits et Les Dessins dans les Maladies Nerveuses et Mentales par J. ROQUES DE FURSAC. Masson et Cie., Paris, 1905. pp. 306.

The value of speech and writing as a diagnostic adjunct to the study of neurology and psychiatry, has been emphasized during the last year by the monographs of Köster and Liebmann, the former relating to graphological disorders without any effort at discussion, the latter to stenographic samples of special disturbances of language. Roques de Fursac, however, has given us a comprehensive and readable treatise on this important subject, although he enters into dogmatic and, at times, tedious discussions. According to him the clinical description of the disorders of writing comprises two points: first, in the material execution of the writing, in its totality, direction and form of lines and the form and dimensions of the letters; secondly, the content of the writing, or the ideas expressed or reproduced by the writer. The study of the first is called caligraphy, of the second psychography. All psychic symptoms, excitements, depression, intellectual enfeeblement, modify the characteristics of the handwriting. The elementary caligraphic disorders relate to the direction and form of the lines, and the direction, dimensions and form of the letters. There is postulated the existence of a special graphomotor centre in the second left frontal convolution, disturbances in this centre giving rise to agraphia and paragraphia. Other symptoms that may arise in various nervous and mental disorders are omission, impossibility of copying, false syntax, substitutions, transpositions, additions, graphic incoherence, echographia, and graphic stereotypy and impulsions. The illustrations are excellent and include samples of writing in paralysis agitans, exophthalmic goitre, chorea, tabes, multiple sclerosis, writer's cramp, the various stages of epilepsy and general paralysis, organic dementia, dementia præcox, acute and chronic alcoholism, various confusional and delirious states, manic-depressive insanity, melancholia, neurasthenia, hysteria, idiocy, imbecility and paranoic states. Especially well shown is the tremor of delirium tremens. One important symptom of katatonia, namely the capitalization of the first letter of each word, seems to have been unobserved by the author. The drawings are very elaborate productions and mostly of the mystic, paranoic type.

Contribution à l'étude de l'Écriture en miroir, par A. LAPRADE. Ollier, Paris, 1905. pp. 58.

If, as has been claimed, mirror writing is the normal chirography of left-handed persons, the subject is closely allied to the dual functions of the brain. The most famous mirror writer in history was Leonardo da Vinci, but this was acquired, as, according to De Beatis, he suffered from paralysis of the right arm. Mirror writing is frequently found in idiots and imbeciles with right hemiplegia or monoplegia and it is not uncommon among left-handed school children. It has been pointed out, that if right-handed persons be asked to write with the left hand with closed eyes, the writing is normal and from left to right and not of the mirror type, thus showing that our memories of letters are visual and not motor. In that writing which goes normally from right to left, as in Hebrew and modern Arabic, the letters seem naturally adapted for this direction. Laprade, in a short monograph, has renewed this interesting question and given abstracts of all the published cases, together with a personal observation, and there is appended a complete bibliography. Pathologically, mirror writing is found to occur in hemiplegia with focal disturbances of reading, writing, and speech, melancholia, hysteria and imbecility. In a case of mirror writing in an imbecile, the patient learned to write before learning to read, and there was, therefore, no means of correcting the error. In the writer's case, in a right-handed man who developed mirror writing following the forced disuse of the right hand as a consequence of a hemiplegia, there was found at autopsy a lesion of the anterior two-thirds and knee of the internal capsule with degeneration of the pyramidal tract and of the facial and hypoglossal bundles. Of course this has no genetic bearing upon the clinical findings.

A Text Book of Psychiatry, by LEONARDO BIANCHI. Authorized translation from the Italian by James H. McDonald. Balliere, Tindall and Cox, London, 1906. pp. xv+904.

Professor Bianchi's "Trattato di Psichiatria" represents the high water mark of Italian psychiatry. It will be welcomed in its English dress if for no other reason than that its system of classification represents a protest against the mental epidemic of Kraepelinism that is sweeping over America. Asylum physicians have bolted Kraepelin whole, without that mental digestion which is the result of independent thinking, and as a result they attempt to force every form of mental disease into Kraepelin's classification. It is unfortunate that Wernicke remains so little known, believing as he does, that in the present state of our knowledge of psychiatry, we are warranted in making only a symptomalogical diagnosis, and that we possess but few clinical entities, *e. g.*, general paralysis, the febrile and toxic psychosis, and the insanities of defective development.

The fundamental principles of the tendencies of various classifications in psychiatry is of interest. The older alienists based their classification on the emotional tone of the patient, and thus arose the much abused terms of mania and melancholia; Kraepelin bases his work on the prognosis of the disease process alone; Wernicke, on a hypothetical cerebral localization; Ziehen, on an association psychology; Bianchi on purely etiological considerations. Personal factors, studies in organic brain affections, pathological anatomy, experiments with drugs in producing artificial mental states, association measured with instruments of precision, have all, in the training of the various exponents of psychiatrical classification, been the contributing factors from which latter evolved each individual system of psychiatry. The ideal can only be arrived at by a harmonious blending of all these criteria, a point not yet reached by any of the varinos systems proposed. But after all, mere classification is not the complete word or the final salvation; the minute analysis of all clinical symptoms should be the effort of every worker in psychiatry, for at present, psychiatry is in the old position of internal medicine before the advent of an accurate pathological anatomy, when many acute diseases were grouped under the generic name of "fevers," because of an accompanying elevation of temperature. The analysis of clinical symptoms alone will solve the perplexing problem of mental disorders, and then perhaps a final classification will arise, if such be the end desired, that will stand the test and onslaught of the various clinical manifestations.

Bianchi's book is divided into two portions: the first comprises the gross and minute anatomy of the brain, together with certain psycho-pathological considerations, the second deals with the special forms of mental disease. The raison d'être for the first part, is well stated in the preface. "To-day, in Italy, which has so largely contributed to the progress of the physiology and histology of the nerve centres, we could not understand a book on psychiatry unless all the corollaries drawn from the various sources were utilized in the interpretation of the phenomena of psychic life. The physiology of the brain, on the solid foundations of morphology and histology, constitutes the heart and nutrient vessels of a medical work on psychiatry." Following along this line of argumentation, Bianchi gives a rapid but fairly minute review of the chief facts of brain anatomy and histology, together with the physiology of the central nervous system in general and of the mechanism of speech in particular. He then passes to an elaborate discussion of the physio-pathology of perception, attention, memory, ideation, emotions, sentiments, will and consciousness. One of the best chapters is that on the methods and field of clinical inquiry. Its scheme, though a little forced and artificial and therefore requiring individual adaption to so flexible a subject as mental disorders, is yet a welcome one, if we consider that the future of psychiatry depends more on minute clinical analysis than on pure pathological anatomy, as the latter has yielded little of value except in a few organic affections. This, in spite of the dogmatic assertion of Nissl, that all mental disorders are but the expression of physical lesions of the cortex, for this will not apply to such diseases as hysteria, the psychasthenias and dementia præcox. In fact, all anatomical findings in the latter disease are secondary, nothing has been detected which can be looked upon as a primary lesion. The method of Ferrari is used for the mental status and for the purely psychological methods, such as reaction time, measure of attention, association, emotions, and measure of voluntary muscular energy, he makes use of the various methods of laboratory technique. In the functional examination of the nervous system, he directs particular attention to the signs of degeneration, a tendency to be expected in Italy, the home of a scientific criminal anthropology. The classification is the most interesting portion of the book, being based as is claimed, on the greatest number of fundamental criteria, as nosological, etiological and anatoma-pathological. Purely etiological considerations, however, enter largely into a number of his descriptions. He divides mental affections in three groups; the *first* comprising those of evolutionary psycho-cerebral defect, the *second* those disorders of infective, auto-toxic and toxic origin developing in individuals regularly evolved, the *third* comprises those affections with a localized or diffuse organic substratum.

In the *first group* are placed the phrenasthenias, the paraphenias, various types of delinquency, epileptic, and hysterical insanity, developmental paranoia, fixed ideas and obsessions, neurasthenia and the sexual psychopathies. The phrenasthenias comprise all those forms of defective mental evolution, such as idiocy, imbecility and cretinism, caused by evolutionary defects, diffuse or circumbscribed pathological processes or by particular forms of intoxication, especially athyreosis. Under paraphrenias are grouped those defects of cerebral evolution manifesting themselves in eccentricity, originality and extravagances.

Delinquency comprises congenital moral insanity and born, acquired, or impulsive delinquents. Under epileptic insanity, he carefully distinguishes those cortical motor explosions which take place without any distubance or consciousness. (the tics.) He believes the various types of developemental paronoia to be entities, but excludes the confusional or acute hallucinatory form which leads to amentia or dementia as well as the systematized deliria following melancholia or the acute psychopathies (constitutional paranoic states, melancholia going into a paranoiac condition, and the paranoiac forms of dementia præcox). Like Kraepelin he considers paranioa as a single, uniform disease, but takes no note of fundamental paranoiac states as postulated and carefully analyzed by Wernicke, neither does he go to the extreme differentiation of Ziehen, who describes ten forms of paranoia. In the chapter on fixed ideas and obsessions, the observations of the French school are closely followed, while under the sexual psychopathies, the trend in the main is that of Krafft-Ebing.

The *second group* comprises psychopathies arising in individuals with a normally evolved brain. This is divided into two *sub-groups* — the *first* being psychoses of infection and autointoxication — the *second* that of the toxic psychoses, the various forms of drug deliria. In the first sub-group he includes mania, lypemania, exalted-depressive insanity, circular and periodical insanity, sensory insanity, mental confusion, acute paranoia, late paranoia, neurasthenic, choreic and luetic insanity, and acute delirium. Why these diseases should be looked upon as having either an infectious or an auto-toxic etiology, is difficult to see, as recent metabolism experiments along these lines, especially the work of Folin and Coriat on general paralysis and the various phases of manic-depressive insanity have yielded nothing of value, or at least nothing which has any strictly etiological bearing. He believes in a pure mania and melancholia, without periodicity, or at least not included in the province of manic-depressive insanity. If there be the occurrence of a similar or opposite phase later, he is still disinclined to include the attack in this group. He admits, however, that pure mania is a comparatively rare disease. His exalted-depressive insanity includes all that group where both melancholia and mania occur, whatever their order. No mention is made of mixed conditions. This complete variance with the clinical entity of manic-depressive insanity, including the manic, depressed and mixed phases as conceived by Kraepelin, together with a disinclination to accept dementia præcox even in its narrowest sense, as will be pointed out latter, is one of the most prominent features of Bianchi's treatise. From the clinical standpoint, the concept of maniac-depressive insanity, completely unifying the old perplexing problems of mania and melancholia, is one of the triumphs of modern psychiatry, and Bianchi's ideas along these lines certainly transcend and contradict all clinical experience. The manic phase with its psychomotor exaltation and flight of ideas, the depressed phase with its psychomotor inhibition and dearth of ideas, the triumph of the concept of the mixed conditions, a peculiar mixture of both states, certainly forms a triad which has stood the test of clinical experience and rigid study, which after all is the redeeming and consistent feature of any new symptom-group in psychiatry. This, together with involution melancholia and dementia præcox in its narrower sense, that is, not as a common dumping ground for all atypical clinical forms of adolescent insanity, are the three disease of the Kraepelin school, which we can afford to bolt whole. They have already undergone mental digestion by our German confréres. Under sensory insanity he includes amentia, acute dementia, dementia præcox, katatonia, stupor and mental confusion. He looks upon them as mere syndromes, as only a part of the morbid picture, claiming that their onset is always with sensory disturbances, hallucinations, or delirium. He does not believe in elevating this syndrome to a clinical dignity. It may be pertinently asked — why? If an onset with sensory disturbances is to be the determining factor of his "sensory insanity," why does he not include under the group the various febrile and toxic psychoses, or even the paranoic states, for these likewise frequently start with isolated hallucinations or illusions? If, as he claims in a previous portion of the book, he has adopted a classification based upon the greatest number of fundamental criteria, nosological, etiological and anatomo-pathological, why does he postulate a group merely upon the content of sensory disturbances in the onset? The contradiction is very manifest. The entire concept of sensory insanity appears to us to be strained and artificial, and totally contradicted by all clinical study in its broadest sense.

Acute paronia, in its delirious or hallucinatory form, is an acute paranoic state, a paranoic delirium frequently with a mystical content. The late paranoias are really the hypochondriacal paranoic states of involution or senility. In the description of choreic insanity, there is no mention of the grave forms of chorea insaniens occurring during pregnancy. Under the *toxic psychoses* of the second subgroup, he includes the mental disturbances of pellagra, alcohol, morphine, cocaine, chloral, lead and carbon monoxide. Alcoholic insanity is given a rather poor clinical description. No mention is made of the exalted or depressive hallucinoses and only a few lines are given to the polyneuritic mental disturbance (Korssakow's disease). Under Saturnine insanity, nothing is said about the lead deliria, or the acute or chronic lead encephalopathies.

Group 3, includes all those diseases which are the expression of known or demonstrable anatomo-pathological alterations of the brain. The descriptions under this group, paralytic dementia, luetic, senile, post-apoplectic, aphasic and traumatic dementia, and the dementia from tumors, scleroses and other organic diseases of the brain, are among the best in the book. The forty-one clinical observations are minute and painstaking, while the illustrations, especially the pathological, leave nothing to be desired.

Histological Studies on the Localization of Cerebral Function, by ALFRED W. CAMPBELL. University Press, Cambridge, 1905. pp. 360.

These new histological studies on the localization of cerebral function, are in many respects so revolutionary and illuminating, that a rather detailed summary of the work seems justified. The greater part of the research was communicated to the Royal Society of London in 1903, and a full publication was made possible by a grant from this same society. Histological studies of the nervous system can be pushed in three directions; by the study of the brain during development, in conditions of disease, and in the normal state. In this case, the normal human material consisted of three cerebral hemispheres examined for nerve cells and fibres, three for fibres only and two partially examined for both. The ages of the individuals ranged from 19 to 48 years. The normal comparative material comprised chimpanzee and orang brains. The pathological material included two brains from cases of amyotrophic lateral sclerosis, seven cases of amputation of extremities, three of tabes, one of an old capsular lesion and two cases of old standing blindness. After some general historical considerations on cortical fibre arrangement and nerve cell lamination, he proceeds to the minute study of the structure and functions of various portions of the cortex. The motor cortex of both man and the anthropoid ape, is limited to the anterior central convolution, the posterior being a "silent" area. This is in harmony with the electrical stimulation experiments of Sherrington and Grunbaum, but in contradistinction to the work of other physiologists. Histologically the area corresponds approximately to the distribution of the giant cells of Betz, cells which alone seem to control volitional muscular movements. In cases of amytrophic lateral sclerosis, a disease limited to the motor neurones, these Betz cells are alone liable to destruction. In cases of amputation also, the changes are limited to the precentral gyrus and the annectant para-central lobule. The post-central gyrus is purely sensory in function, constituting the higher cortical terminus for the conveyance and appreciation of impressions relating to the complex tactile sense. In this area there are no true cells of Betz. It seems to correspond to the Monakow's cortical lemniscus. In a disease primarily of the sensory neurones, namely, tabes dorsalis, the cortical cell changes are absolutely limited to this region, and in those conditions where the common sensations suffer a disturbance (tactile, muscular, stereognostic, pain, temperature), the histological changes found in the brain are also confined to the post-central gyrus. Histologically he distinguishes between the visuo-sensory and visuo-psychic areas, the functions of which are sufficiently indicated by their names. Both are limited to the calcarine region, a conclusion derived from ablational experiments and clinico-pathological material (cortical or subcortical hemianopsia, distribution of macular field, psychic blindness, alexia, color blindness, optic aphasia). The area of the cortex for the reception of auditory stimuli covers the transverse temporal gyri and in the recorded cases of long standing total cortical deafness, this area was included in the destruction, together with neighboring parts like the insula, supramarginal gyrus and the opercular part of the ascending parietal convolution. Its histological structure is homologous with the visual area. In cases of pure word deafness, he was unable to detect any microscopic changes in the left angular gyrus, but in the literature relating to this condition, there was found atrophy of the temporal lobes. The auditory neuronic chain is very complex, comprising the end fibres from the organ of Corti, the cochlear nerve, the ventral and dorsal cochlear nuclei as the first link in the chain; the striae medullares, the corpus trapezoideum and the superior olivary bodies as the second link; the retrolentiform portion of the internal capsule, the corona radiata and the temporal cortex as the third link. In conditions where the sound-perceiving or the sound-discriminating sense is impaired, any of these links may be disturbed. The physiological centre for the sense of smell is confined to the limbic lobe, the lobus pyriformis and the cornu ammonis. Clinico-pathological data have only partially borne out this evidence, however, but comparative anatomy has shown that the parts detailed are the primary olfactory centres, in spite of the contradictions seen in anosmatic animals. His "parietal" area comprises the precuneus, the superior parietal gyrus and the anterior part of the supra-marginal gyrus. Its function seems to be that of elaborating the complex impressions of the muscular and the stereognostic senses. The "intermediate pre-central" field is in front of the pre-central area proper and extends downwards to the orbital surface of the hemisphere. It includes the well known area of Broca, the centre for speech. He partially postulates a separate writing area in this field, as he found cell changes in a case of amputation of the right hand, thus agreeing with Bianchi's theory. One of its chief functions seems to be the control of high and low evolutionary movements, this being so well marked that he states the following "Law:" "in the intermediate pre-central cortex there is a sequential deposition of centres for the control of higher evolutionary movements, following the same order from above downwards as that observed in the precentral area proper." The frontal and prefrontal areas are electrically "silent;" they seem to be the seat of the high cerebral functions, making up the "psychical tone" of the individual. Removal or disease seems to disaggregate the personality, peculiar forms of mental disturbance and alterations of character occur, the "Witzelsucht" of the German writers. In cases of dementia, the greatest cerebral wasting occurs in this region, varying directly with the amount of mental defect. In other words, the higher psychic and association functions seem to be localized in this area; witness the atrophy in idiocy, imbecility, general paralysis and in some cases of dementia præcox. The data of comparative anatomy also bears out this hypothesis. The island of Reil is phylogenetically very old, it probably represents the gustatory centre and it has been found altered in cases of aphasia. The addendum comprises the comparative anatomy and physiology of the brains of the cat, dog and pig. The plates are well drawn and graphically represent the views of the writer on all the points discussed. In view of the thorough work, it is to be hoped that the author will apply his methods (complete studies of serial sections over entire areas) to the study of the basal ganglia, the deeper parts of the cortex and various nerve tracts. I. H. CORIAT.

The Analysis of Racial Descent in Animals, by T. H. MONTGOMERY Henry Holt & Co., New York, 1906. pp. 311.

The author discusses environmental modes of existence, hereditary and embryonic differentiation, relations of modes of reproduction and conjugation, life cycles and polymorphism of individuals, variation and mutation, transmutation of species, parellelism of ontogeny and phylogeny, morphological comparisons, relative values of morphological characters and criteria of racial advancement. Each vital phenomena he considers a step in the individual or racial change. If the morphologist regards structure as a visible stage of progress, there can be no conflict between him and the physiologist. If we knew what form meant we should interpret it into function. Interpretations of descent have hitherto been too morphological. They should include chemical and physical constitution and environment, and especially the relative value of characters implied, the criterion of which is the degree of conservatism, should never be lost sight of. We must assume monophyletic origin until the opposite is proven. We must anticipate intermediate connectants between species, must consider modifications due to stimulating changes in the environment, and deem hereditable substance not excluded from external influence. The individual does not recapitulate the development of the race, and no particular ontogenic stages are more ancestrally remnant than others, but all stages of ontogeny are equally cenogenetic and palingenetic. To become inherited, a modification must produce changes in the energy of the germ plasm. All comparisons between diverse mechanisms are inexact analogies, but in most phylogenetic research we must base upon these. The unit of comparison is the whole individual during its life history and not any selected stage of it. The author inclines very strongly to Hatschek's trochophore theory, viz., that the free larvæ of marine annelids show uniformity of structure and these are all modifications of one kind. The earlier the modifications appear the more apt they are to be inherited. If the free swimming larvæ were a repetition of an ancestral adult condition, why should it not be equally conserved in marine and fresh water life?

Die Ameise, von K. ESCHERICH. Vieweg & Sohn, Braunschweig, 1906. pp. 232.

The author has here gathered the main results of the studies of nearly half a century and presented them in a systematic, critical way with 68 illustrations. The one great family of formicidæ comprises over five thousand species, sub-species and varieties. There are 170 genera and 5 great sub-families. There are enormous variations. The distribution of ants is almost worldwide between the polar circles. Although their optimal habitat seems to be the tropics, they have sometimes transcended both the Antarctic and the Arctic circle. They have a very common trait of founding states or colonies and the sterile female is greatly in evidence in all the species. While ants are no miniature men, they are no mere reflex automata, but have the psychic qualities of memory, association, perception, utilization of individual sense experience, and thus power of individual plastic adaptation. The latter is most pronounced among the workers, less with the queens and is almost undeveloped in the males. This difference has a very marked reflection in the structure of the brain which differs very much, the gray matter being far more developed in the workers. Something is known of their phylogeny. They long antedated man. Our author thinks that the first ants were winged and that they very slowly lost this trait, but that some species have reverted to wings, remarkable as this is. He also assumes that polymorphism had a slow development and is largely tropogenic. Wasmann has shown that the appearance of pseudogyns has a casual relation with the presence of certain guests of ants. These lived with them first in the symphilia. They were first received, they grew up in the nests and last the ants cared for them. These later destroyed the eggs and larvæ of their hosts. When the first workers appeared, there was, of course, an important change in the habits of these ants. Another phyletic trait is the slow development of the *Pilzgarten*. The writer believes that we can trace pretty directly the development of every stage of this process. He also has something to suggest about the instinct of feigning death, but the most important phyletic contribution is the writer's theory of eight stages in the development of the colonies, illustrated by as many different species. In this way he accounts for slavery, domestication and the development of mixed colonies. He deems that his results are borne out by the experiments of mixing species. On the whole this must be called quite a masterly compend.

Die Mimik des Denkes. Von DE SANCTIS. Halle, 1906.

De Sanctis reminds us that the reflexes, pulse, respiration, the pupil, respond with exquisite sensitiveness to psychic states. Thought mimesis comprises first that of sensory attention and second more intellectual inner reflection. The apparatus chiefly involved is the seventh pair of facial nerves, the root of which arises between the pons and the medulla. Whether a cortical or bulbar origin is manifold or simple, probably neither Mendel's view that it is bulbar nor that of Monakow or Wildebrand that it is central should be neglected. Usually both are synergetically associated. Where the mimicry is purely voluntary the impulse probably originates from the psychomotor cortex. In childhood and in old age or in agenesia this is not developed. Animals differentiate their mimetic movements far less than man and their mimesis of attention has less emotional reinforcement. The higher animals have no proper centre for attentive mimesis. It may be in the ears or in the mouth. Sometimes it is a general irradiation and the whole body is motionless or tense, but with animals as soon as the tension becomes great it takes on an emotional character and spreads to the whole body, showing that attention is but little developed and that intellectual activity is not canalized in paralyzed nerve excitability. In old age mimic innervation loses the power to express transient effects by sufficient complementary movements, but only expresses the fundamental results in grosser movements. Old men in thinking are hypomimetic in comparison with children. Art shows the same thing.

In adults, thought mimesis is well differentiated from that of emotion, and in those accustomed to think, this difference stands out yet more clearly. In a certain sense this mimesis of thought in adults has its own organ, viz., the attentive mimic centre in the upper half of the face. This is often unsymmetrical. Its intensity is usually weak in brain workers, especially in those who have to tense the eyes its vigor is greater; in those who cannot read it is less. A very common irradiation is the mouth, but the individual variations are very great as is the toning of the effective elements. Very often we find positive anomalies which are hard to explain. They may extend not only to the back but to the limbs, head and the whole body. The mimesis of the sensory and inner attention is chiefly optical. Even the other senses express themselves in the visual sphere.

The mimesis of concentrated differs very much from that of diffuse thought. In the former the gestures and attitudes arise in the periphery and converge to the centre, especially the eye, as in the meditative type. Often the lower half of the face is hypertonic. This rigidity often distinguishes meditation from sleep. The muscles of the forehead are closely drawn together. The different types may vary according to the object, the habits of the subject or the degree of concentration. Some concentrations have a happy and some a sad nuance. The Japanese gods meditate with a smile. Philosophic thought is more often sad. The mystic illumination and unification are rapt as the writer shows by many quotations. The eyes may be rolled toward heaven or fixed upon some indefinite distance, and very often there is a peaceful and heavenly expression.

In diffuse thought and abstraction the attention is centrifugal. In dreamery, reverie, art, Donders and Darwin both find vacant expressions in the eyes. The axes may diverge and are not fixed. Donders found actual divergence in some cases. The physiognomy is often that of enchantment and the expression is more often happy than sad. Sometimes it is not unlike that of sleeping children; so in ecstasy and hallucination. This is often connected with love and monoideism and Buddhism, with synderesis, or unified with the absolute or Nirvana. The expression may be hyperasthenic and the wishing and longing may reach a painful intensity. All ecstasy is immobile and involves suspense and silence. God, says Meister Eckhardt, created the world in order to rest in contemplation. The Italians love to contemplate the actual visual world.

There are, of course, many modifications by race, sex, custom, age, and especially disease. The mimesis of attention in the blind is more fugitive, partial and less energetic than in normal people, and the contraction is often limited to a single group of muscles. The facial movements of the blind when reading aloud by touch are usually motor accompaniments of the oral expression and are weaker when they do not read aloud and vanish in inner attention, *e.g.*, arithmetic. All blind persons, when attentive, are more or less motionless, at least in head and face. In many, spontaneous attention causes tension of the muscles in the back of the neck which become stiff. The difference between those born blind and those who become blind later is that in the former the contraction of the muscles of the forehead and the orbiculares palpebrarum in commando exercises is very difficult, and those of the superciliary muscles are impossible, but by those blinded late in life the isolated contraction of the frontalis and obiculares is still possible, though it be often weak and sometimes that of the superciliary muscles may be possible even by itself alone. In anger and pain, all blind persons contract their tension muscles, but less than do normal persons.

The intellectual expression of thought, then, by adults is chiefly in the mimic eye zone and in its three dermal muscles. The latter have manifold phylogenetic and ontogenetic functions besides the expression of thought, but the superciliary is most set apart for this latter purpose. Duchenne thought that the frontalis was for attention, the orbicularis and the superciliary muscles for reflection. Most essential are the superciliary muscles which are completely expressive, though not exclusively devoted to attention. They are developed in animals and children to protect from light and are tense in heat, short-sightedness, anger, sadness, etc. Thus the original purpose of the ciliary muscle is to protect the eye. The superciliary muscle, therefore, has three functions. Its highest may be alone and independent of the other functions and other muscles. It makes the vertical furrows on the forehead. Charles Bell thought animals lacked it. How could its pain function be transmuted into thought. Darwin connected it with the child's cry, frowning being its last trace. It narrows and sharpens the field of vision according to his principle of the association of purposive habits. This resembles Wundt's or Piederit's association of analogous adjustment of sensations. "The muscular movements of expression relate to imaginary sense expressions." Thinking always causes effort, if not pain. It is the expression of the psychic. Thus the pessimist has some basis, if thought is pain. Thus to adjust the eye to light in the child is the basis of thought expression in the adult. Sensorial optical attention is its genetic foreschool. Some psychologists think attention is always moving; others that it is fixed, but both theories are needed. In even the best of us, attention and thought are not entirely free from emotion. Thus movement and inhibition act and react upon each other, but while there must be tension there must also be rest and silence. In thought, respiration and vasomotor activities diminish; the muscles are a little relaxed; the pupils widen; blood pressure changes. In general, mental work is thus very different from physical.

Physiology of the Nervous System, by J. P. MORAT. Authorized English edition translated and edited by H. W. Syers. 263 illustrations. W. T. Keener & Co., Chicago, 1906. pp. 680.

This great work is a part of the more comprehensive treatise on physiology by Morat and Doyon. The author premises that it is the nervous system which decides at what moment the energy accumulated by the living being shall be liberated, that is, shall leave matter and exert its motor functions. This point it decides with the aid of the senses and by often a lengthy inner elaboration. The cycle of nervous current implies impression from without, sensation within, and motor response, and lastly changed impressions due to movement. In the nervous system all movement induces sensation and all sensation induces movement. It has a marvellous attribute of adjourning its events until the appropriate moment. From the fact of the introduction of sensation into the cycle, events assume a peculiar meaning. The tonality is either pleasurable or painful, the former always preferred to the latter. Sensation seems to modify the relation between cause and effect. External events are preserved by being reduced to representation. It is a false impression that the end and aim of an act is its cause, for the latter must precede and not follow the former. Thus physiology gives rise to psychological problems which are out of its domain. The author first treats of sensibility in its relations to energy, determinism, organization, excitability and reaction; he then distinguishes between static and dynamic unity, and the first chapter starts with an account of the static condition of the neurons, including their dynamism, individuality, forms, functions, degeneration and stimulation; then the energies of the nerve with the current of repose, action and negative variation, fatigue, electrotonus, laws of contraction and nerve poisons, are discussed. Under organization the author treats sensibility and movement in their relations, beginning with nerve pairs, and then discussing spinal nerves and metamerism, cranial or sensory nerves, their inter-relations; then the glossopharyngeal, pneumogastric and the hypoglossal. In the next chapter the indication of impulses and the reflex act, together with inhibition, conservation of stimuli, etc., are discussed. Then come consciousness and unconsciousness, animal and organic life, cerebralization of the stimulus, respiration, circulation, secretion. The next chapter treats of orientation, equilibrium, the emotions, intelligence in its relation to the brain, localization, etc. Then come specific innervations, tactile, visual, auditory, olfactory and gustatory, and lastly language and its defects, idealization, association and sleep. Altogether it is a book of very great importance, is well up to date and should be on the reference shelf of every psychologist. Its 263 illustrations are well chosen and the index and literature, so far as we have observed are well made.

The Subconscious, by JOSEPH JASTROW. Houghton, Mifflin & Co. Boston, 1906. pp. 549.

Professor Jastrow has performed a very useful service in bringing together the results of the recent very copious literature upon the unconscious and presenting it with many well chosen quotations in an interesting way which is at once scientific and popular. The book is timely, will be welcomed and read by every psychologist in the land, and probably will, as it certainly should, have a large sale among the rapidly growing class of laymen interested in the subjects it treats. He divides his chapters into three parts—normal, abnormal and theoretical. He first treats the function of consciousness, its relation to the nervous system and to volition and its mechanism and the distribution of attention, the subconscious in mental procedure and in maturing thought, lapses of consciousness, and finally self-consciousness itself. The second part seeks to define the range of the abnormal; discusses dream consciousness and its variants, dissociation, the genesis of altered personality and its disintegrating lapses. The third treats of the general concept of the subconscious, its abnormal forms, and draws conclusions.

It is impossible here to do justice to a work so lucid, comprehensive and many-sided, for only an epitome of the whole would be adequate. The author's general conclusion may, however, be summarized. Man does not live by consciousness alone, for older and deeper than it are the dispositions which make the basis upon which it has been developed, meet some needs not adequately provided for by inherited endowment. Its supreme function is the integration of experience. Although liable to disintegration it is essentially a unifying function. The author sees the intimate relation of subconscious activity to mental evolution which is its only key. He recognizes that while the lines of tendency converge toward one normal product the paths of dissolution are puzzlingly divergent. Thus he does not accept as fundamental any scheme of conflicting personalities. He believes that the soul is full of short-circuit processes, so that experience is a mixture of long- and shorthand characters that are not stenographic records of experience at all, but are an independent alphabet. On this view, hypnosis shows a power of knowledge revealed below the threshold that has no origin in the experience of the individual. The theory of a subliminal self, however, is not entireiy satisfactory. It could hardly be explained as atavistic because this means survival from below and not culling from above. The practical point of it all is that consciousness and endeavor should occasionally be allowed to lapse and we should allow "the surgings from below to assert their influence," or we should cease to strive and fall back upon the corrective support of the unconscious. "The knowledge that is conscious goes and the wisdom lingers in the subconscious traits of character."

The Problem of Psychiatry in the Functional Psychoses, by EDWARD COWLES. American Journal of Insanity, Vol. LXII, No. 2, October, 1905, pp. 189-237.

Dr. Edward Cowles occupies a unique position to-day in American psychiatry. The founder of the first school for nurses for the hospitals for the insane, the pioneer in this country in introducing experimental methods into the hospital itself, a tried administrator for many years of perhaps the richest and the most elegant hospital in the world and an original contributor to his department, he occupies to-day an eminent and enviable rank, and it is to be greatly hoped that he will bring to maturity and to systematic presentation his own manifold studies. By a singular irony of fate, the very man who was the first in this country to see the importance of establishing clinical and scientific laboratories where chemical, neurological and even psychophysical tests and experiments could be made, and who led this movement, was, at one time, a little in danger of being regarded by a few of the younger men, some of whom had been brought up under him, as critical of their own extreme structural methods of interpretation. A wave of very injudicious economy in the board he had so long served prompted him, at about the same time, to withdraw from it. If he had yielded at this rather discouraging point, it might almost have been said of him as of Jubal, who first taught his people music and was later rejected from a great concert, so that forgotten he lay down to die, while his great art and even his name filled the sky. But this is not what happened, for Dr. Cowles has with great discretion and courage kept on writing and growing. His view as defined in the above article seems to us to represent about the sanest view to be found in the whole field and the view that is to prevail. More than this, psychiatry is now taking a rather sharp and sudden trend, so that whereas structure has led and been dominant, now function is coming to the fore. The old dogma that no psychic disease was known or even established unless the post mortem findings showed some lesion is everywhere giving way to a broader and more psychological view. Joinet's visit to this country was most opportune in this respect and there is a prospect that morphology and the study of brain lesions will be relegated to their true and just position. This is precisely what Dr. Cowles has stood for, as all those who have known his career for many years will testify, and psychologists as well as the most progressive psychiatrists now acknowledge the eminent value of his services. This paper, which it is hoped is only the first of a series setting forth in greater fullness his views, discusses precisely these issues, between those who study insanity chiefly from the cerebral side and those who approach it chiefly from the functional side.

The Evolution of Knowledge. A review of Philosophy. By RAYMOND ST. JAMES PERRIN. The Baker & Taylor Co., New York, 1905. pp. 308.

This book originated in a student production in 1881 in which the author aimed to show that space, time, matter and force can be resolved into motion. Since then several other works have elaborated this thesis and here we find motion to be the ultimate reality. In the first part of the present book, he traces this idea from Thales down to Cousin, Comte, Reade and Hamilton and in the second part he describes the evolutionary philosophy of Herbert Spencer and George Henry Lewes. The author is a vigorous believer in religion, regrets that specialization has separated the forces of instinct and reason, but believes they are destined to reunite in the evolution of knowledge. Old creeds fail to inspire us because they have ceased to represent nature. The chief enemies of the church are zealots. The central problem of education is the proper use of fiction. Yet the imagination should not be stimulated at the expense of the truth.

Christianity and Sex Problems, by HUGH NORTHCOTE. F. A. Davis Co., Philadelphia, 1906. pp. 257.

This is a truly remarkable work. It treats sexuality in childhood, the mixing of the sexes in schools and elsewhere, the battle of chastity in the adult, neo-Malthusianism, fornication, venereal diseases in legislation, marriage, modesty, divorce, the sexual in art, sex perversion, impure language, the gospel and sex relations, etc. These titles, however, give little idea of the contents of the book. The author wishes to see a new ethics of the sexes developed and thinks it is being slowly evolved. It is a hard study, implying knowledge of anthropology, biology, medicine, law, theology, psychology, etc. The student must go through many pages so vile as to try his nerves, and needs to be highly endowed with the moral qualities of tact and caution for the subject is a dangerous one. The sexual evil of our day is painted by Northcote in very lurid colors, but our author's treatment of the whole subject throughout is permeated with a religious sentiment. He believes that it is one, if not the chief, function of religion to keep this passion pure and exalted and hold it true to its purpose. While he has himself evidently gone through the most repulsive chapters, this study has not contributed in the least to diminish his reverence—nay, almost worship—for the subject. He sees educational possibilities here higher perhaps than any one else has yet detected, and holds that religion and sexual life must go together, that they rise and fall with each other, and either can be kept pure only by the other. There is much plain language in this book, but, on the whole, none of the plain books on this subject are perhaps better suited to the needs of, let us say, thoughtful men of collegiate grades.

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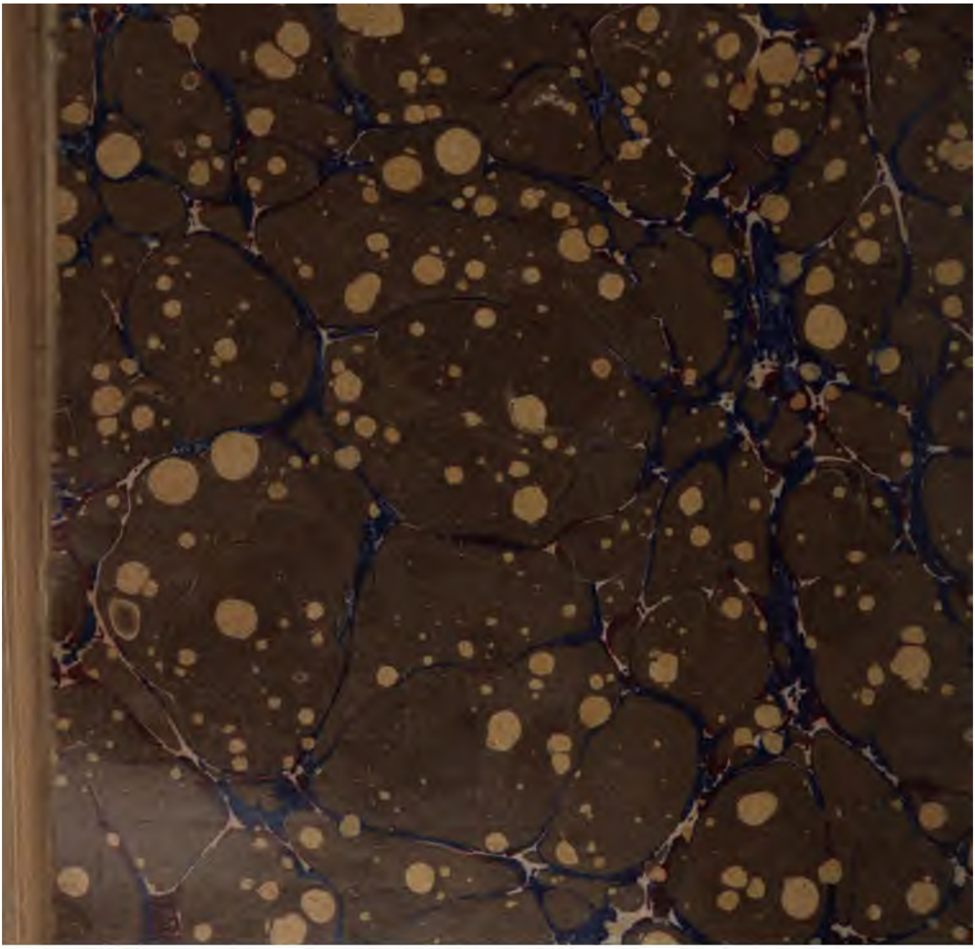

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